PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXVII. For the Year 1787.

PART I.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of a new Comet. In a Letter from Miss* Caroline Herschel *to* Charles Blagden, *M. D. Sec. R. S.*

Read Nov. 9, 1786.

SIR,

IN consequence of the friendship which I know to exist between you and my Brother, I venture to trouble you in his absence with the following imperfect account of a comet.

The employment of writing down the observations, when my Brother uses the 20-feet reflector, does not often allow me time to look at the heavens; but as he is now on a visit to

Germany, I have taken the opportunity of his abſence to *ſweep* in the neighbourhood of the ſun, in ſearch of comets; and laſt night, the 1ſt of Auguſt, about 10 o'clock, I found an object very much reſembling in colour and brightneſs the 27th nebula of the *Connoiſſance des Temps*, with the difference however of being round. I ſuſpected it to be a comet; but a hazineſs coming on, it was not poſſible intirely to ſatisfy myſelf as to its motion till this evening. I made ſeveral drawings of the ſtars in the field of view with it, and have incloſed a copy of them, with my obſervations annexed, that you may compare them together.

Auguſt 1, 1786, 9 h. 50′, the object in the center is like a ſtar out of focus, while the reſt are perfectly diſtinct, and I ſuſpect it to be a comet. Tab. I. fig. 1.

10 h. 33′, fig. 2. the ſuſpected comet makes now a perfect iſoſceles triangle with the two ſtars *a* and *b*.

11 h. 8′, I think the ſituation of the comet is now as in fig. 3.; but it is ſo hazy that I cannot ſufficiently ſee the ſmall ſtar *b* to be aſſured of the motion.

By the naked eye the comet is between the 54th and 53d Urſæ majoris, and the 14th, 15th, and 16th Comæ Berenices, and makes an obtuſe triangle with them, the vertex of which is turned towards the ſouth.

Auguſt 2. 10 h. 9′, the comet is now, with reſpect to the ſtars *a* and *b* *, ſituated as in fig. 4. therefore the motion ſince laſt night is evident.

10

* A doubt having ariſen about the identity of the ſtars marked *a* and *b* in the figures, I have examined that part of the heavens in which the comet was the 1ſt of Auguſt, in order to ſettle this point, but find ſo many ſmall ſtars in that neighbourhood that I have not been able to fix on any of them that will exactly anſwer

10 h. 30′, another conſiderable ſtar *c* may be taken into the field with it, by placing *a* in the center; when the comet and the other ſtar will both appear in the circumference, as in fig. 5.

Theſe obſervations were made with a Newtonian ſweeper of 27 inches focal length, and a power of about 20, the field of view is 2° 12′. I cannot find the ſtars *a* and *c* in any catalogue; but ſuppoſe they may eaſily be traced in the heavens; whence the ſituation of the comet, as it was laſt night at 10 h. 33′, may be pretty nearly aſcertained.

You will do me the favour of communicating theſe obſervations to my brother's aſtronomical friends.

I have the honour to be, &c.

CAROLINE HERSCHEL.

Slough, near Windſor,
Aug. 2, 1786.

anſwer theſe figures; and as they were drawn from obſervations made by moonlight, twilight, hazy weather, and very near the horizon, it would not be at all ſurpriſing if a miſtake had been made: however, as theſe figures were only given with a view to ſhew the motion of the comet, the concluſion of the change of place, which was drawn from them, was equally good whether theſe ſtars were the ſame or different.

Dec, 14, 1786. WILLIAM HERSCHEL.

II. *Remarks on the new Comet. In a Letter from* William Herſchel, *LLD. F. R. S. to* Charles Blagden, *M. D. &c. R. S.*

Read Nov. 16, 1786.

DEAR SIR,

AS my Siſter's letter of the 2d of Auguſt, relative to the comet diſcovered by her, has had the honour of being communicated to the Royal Society, I beg leave to add the following remarks upon it.

The track of the parallel not being taken at the time of her obſervations, I have endeavoured to recover it by means of directing the ſame inſtrument which was uſed on this occaſion towards that part of the heavens where it was placed the 1ſt and 2d of Auguſt. Hence, from the annexed figure (ſee Tab. I. fig. 6.) in which A, B, repreſents a parallel of declination, we may conclude, that the comet was nearly in the ſame meridian with the ſtar *a*; but more north than it by an interval equal to the diſtance of the ſmall ſtar *b* from *a*. This will conſequently give us a pretty good opportunity to aſcertain the comet's place with ſome accuracy.

I have the honour to be, &c.

WILLIAM HERSCHEL.

Slough, near Windſor,
Nov. 15, 1786.

P. S.

Fig. 1.

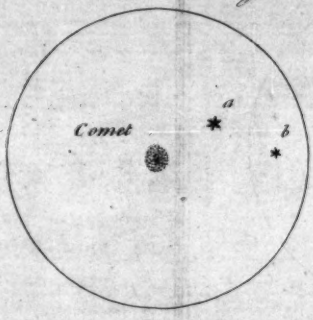


Fig. 4.

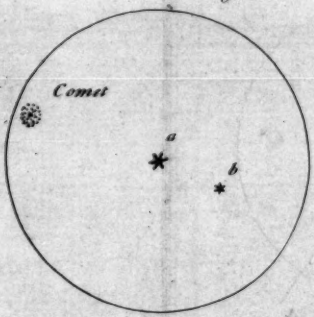


Fig. 2.

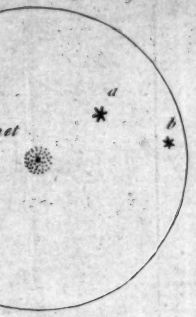


Fig. 3.

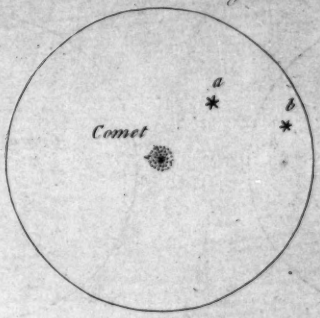
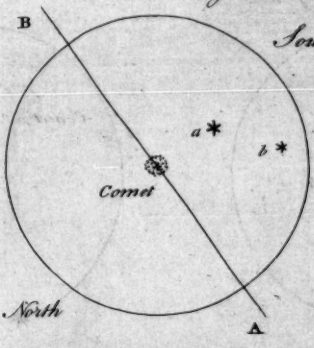


Fig. 5.

Fig. 6.



Rogers sc.

P. S. The first view I had of the comet, after my return from Germany, was the 19th of August, when with a 10-feet reflector it appeared not much unlike the third nebula of the *Connoissance des Temps*, with which it might be very conveniently compared on account of its proximity. It was, however, considerably brighter, and seemed to have a very imperfect and confused kind of gathered light about the middle, which could hardly deserve the name of a nucleus. It had also, besides a diffused coma, a very faint, scattered light towards the north following part, extending to about three or four minutes, and losing itself insensibly.

III. *Magnetical Experiments and Observations.*

By Mr. Tiberius Cavallo, *F. R. S.*

The Lecture founded by the late HENRY BAKER, Esq. F.R.S.

Read Nov. 16, 1786.

THE Bakerian Lecture, which last year I had the honour to deliver to the Royal Society, contained the account of some magnetical experiments, particularly concerning the magnetism of brass, from which it appeared, that most brass becomes magnetic, so far as to attract the magnetic needle, by being hammered, and loses its magnetism by annealing or softening in the fire; but that there is some brass, which possesses no magnetism naturally, nor acquires any by hammering.

Several experiments, made since the reading of that paper, having shewn a few particulars, which tend to correct what was advanced in the said Paper, I shall in the present lecture mention, first, those particulars, and shall then proceed to relate other experiments and observations relating to other branches of the same subject of magnetism.

In performing the experiments on the magnetism of brass, I generally used a magnetic needle suspended in a peculiar manner, as it is described in my last lecture; *viz.* a common sewing needle, or a piece of steel wire rendered magnetic, and suspended

ſuſpended by means of a chain of hair; which ſort of ſuſpenſion I find not only from the experiments then made, but alſo by ſeveral ſubſequent trials, to be the nimbleſt hitherto contrived; becauſe ſome ſubſtances which ſeem to be quite deſtitute of magnetiſm, by not attracting any of the magnetic needles otherwiſe ſuſpended, will ſenſibly affect this. However, notwithſtanding the nicety of this method for diſcovering a very low degree of magnetic attraction, it was found ſtill inferior to that of exploring ſubſtances floating on the ſurface of quickſilver, as uſed by M. BRUGMAN*. It ſeemed, therefore, neceſſary to repeat ſome of thoſe experiments on braſs, and alſo upon platina, by examining their magnetiſm by this means, *viz.* by putting the pieces of braſs or grains of platina upon the ſurface of quickſilver, and then preſenting a ſtrong magnet near them. The reſult of thoſe experiments was, that very ſeldom a piece of braſs, or grain of platina, occurred, which was not affected by the magnet; and even when they were not affected by it, their indifference, as may be expreſſed, was not very clear and deciſive; and indeed there are very few ſubſtances in nature which, when examined by this means, are not in ſome degree attracted by the magnet, ſo general is the diſperſion of iron, or ſuch is the tendency which moſt bodies have towards the magnet.

Such braſs which in the former experiments appeared to have no magnetiſm naturally, nor to acquire any by hammering, was now found to be moſtly magnetic, though in ſo very ſmall a degree as to be diſcoverable only when floating upon quickſilver. The ſame was the caſe with the grains of platina before they were hammered; but after hammering

* See his Magnetiſmus, ſeu de Affinitatibus Magneticis, printed at Lyons in 1778.

their

their attraction towards the magnet became more evident; whereas thoſe pieces of braſs, which naturally had not any degree of magnetiſm ſufficient to affect the needle, nor acquired any by hammering, but yet ſhewed ſome tendency towards the magnet when floating upon quickſilver, never, or very ſeldom, had that tendency increaſed by hammering.

As in the courſe of thoſe experiments it naturally occurred to obſerve ſeveral particulars, which may be of uſe to thoſe perſons who wiſh to repeat theſe experiments, I ſhall now ſubjoin the principal of them.

It is neceſſary firſt of all to obſerve, that the veſſel wherein the quickſilver is put for the purpoſe of examining the magnetiſm of divers bodies, muſt be at leaſt ſix inches in diameter; otherwiſe the ſubſtances that are ſet to float upon the mercury, will be continually running towards the ſides of the veſſel, on account of the curvature of the ſurface of that metal, which in narrow veſſels begins from a greater diſtance from the edge, than in veſſels of a larger diameter.

It is neceſſary likewiſe to keep the quickſilver very clean, and alſo very pure; the want of which precautions will render the event of the experiments precarious. I have obſerved a very remarkable phænomenon, with reſpect to the ſurface of the mercury that is uſed for this purpoſe. It is, that though ſubſtances will float upon it with wonderful nimbleneſs, when the mercury is firſt poured out of the bottle into the open veſſel, yet a ſhort time after, *viz.* after having remained for an hour or two, and ſometimes for a ſhorter time, expoſed to the atmoſphere, a piece of braſs or other ſubſtance will by no means float upon it with equal facility; ſo that ſome pieces of braſs, or grains of platina, which, after firſt pouring the quickſilver into the open veſſel, were evidently attracted by the magnet, about

about an hour after were not in the leaſt attracted by it. The method which, when the ſurface of the quickſilver is rendered thus ſluggiſh, will effectually purify it, is to paſs the quickſilver through a funnel of paper, *viz.* a piece of clean writing paper rolled up conically, and having at its apex an aperture of about a fiftieth part of an inch in diameter; which operation is ſometimes neceſſary even on firſt pouring out the quickſilver, and which I have often been obliged to repeat three or four times in the courſe of an hour. There ſeems to be formed a kind of cruſt upon the ſurface of the mercury when expoſed, which, though inviſible by mere inſpection, may be perceived by moving the floating body; for if it be tried immediately after having paſſed the quickſilver through the paper funnel, the floating ſubſtance will ſeem to proceed by itſelf; whereas, ſome time after, the ſame body, when moved, ſeems to communicate that motion to the adjacent quickſilver, and to drag it along with itſelf, ſomewhat like when one moves a body, which floats upon the ſurface of a liquor that begins to coagulate.

The formation of this cruſt ſeems to be moſtly owing to the imperfect metals, which in various quantities are uſually amalgamated with the common ſort of quickſilver; becauſe that amalgamation tends to dephlogiſticate thoſe metals, and the ſemicalcined part floats at the top, and it is not unlikely that the dephlogiſtication goes on much quicker in the open air. The reality of this ſuppoſition is corroborated by obſerving, that the purer the quickſilver is, the ſmaller is the cruſt formed, or oppoſition made to the floating bodies. However, I have obſerved it in ſome meaſure even in the pureſt quickſilver that can be procured; and am inclined to think, that it muſt be

partly owing to moiſture, or to very fine duſt that adheres to the quickſilver when expoſed to the atmoſphere.

In performing ſuch experiments care ſhould be had to keep the air and the quickſilver as much undiſturbed as poſſible, and alſo to preſent the magnet in a proper manner, *viz.* ſo as not to touch the ſurface of the mercury, nor to ſtrike againſt the floating body, eſpecially when that is in motion; for after the impulſe, though that may be very ſlight, the floating body will be impelled backwards, which may be often miſtaken for magnetic repulſion. The leaſt exceptionable method is the following: firſt, if the floating body be in motion let it ſtop, then hold a ſtrong artificial magnet nearly in a perpendicular direction, and with one pole juſt over one ſide of the floating body, or rather ſo that the perpendicular, let fall from the pole of the magnet to the ſurface of the quickſilver, may be about one-tenth of an inch diſtant from the body to be tried. The height of the magnet above the quickſilver ſhould be juſt ſufficient to let the floating body paſs under it without touching it. In this ſituation the magnet muſt be held ſteady; and if the floating body has any magnetiſm, it will be ſoon drawn directly under the magnet.

In theſe experiments it will be generally found, that one part of the floating body is more magnetic than the reſt, which appears from that particular part being conſtantly drawn directly under the pole of the magnet; whereas, when the magnetiſm is diffuſed equably, the center of gravity of the body (provided its ſhape be not very irregular) becomes ſtationary juſt under the pole of the magnet.

It is not every magnet that will diſcover the very weak magnetiſm of certain ſubſtances; for ſometimes a powerful mag-

net

net will evidently attract what a weaker one will not move in the leaſt.

I ſhall laſtly obſerve, with reſpect to the experiments of laſt year's lecture, that though then I thought to have fuſed and incorporated together braſs and iron, yet ſome ſubſequent trials gave reaſon to believe, that the iron is concealed in ſome part or other of the melted braſs, rather than equably diffuſed through the ſubſtance of the latter; and the principal reaſon for this ſuſpicion is, that when thoſe pieces of mixed metal are tried upon the quickſilver, ſome points in their ſurfaces are generally attracted by the magnet in preference to others.

Experiments and obſervations relating to the attraction between ferrugineous ſubſtances and the magnet, in their different ſtates of exiſtence; to which are added, ſome thoughts concerning the cauſe of the variation of the needle.

It is a propoſition well eſtabliſhed in magnetics, that ſoft iron, or ſoft ſteel, acquires magnetiſm very eaſily, and loſes it with equal facility; but that hard ſteel acquires that power with difficulty, and afterwards retains it obſtinately. From the conſideration of thoſe properties I was led to imagine, that if a piece of ſteel, whilſt red-hot, were placed between magnetic bars, and whilſt ſtanding in that ſituation cold water were to be ſuddenly poured upon it, ſo as to harden it, there might, perhaps, be obtained an artificial magnet, much more powerful than what can be produced by the ordinary way; becauſe the magnetic bars, employed for ſuch purpoſe, would communicate a great degree of magnetic power to the ſteel, when red hot and conſequently ſoft, which power would be fixed upon the ſteel by the hardening.

In order to put this project to the trial, ſix magnetic bars were ſo diſpoſed in an oblong earthen veſſel, as that the north poles of three of them might be oppoſite the ſouth poles of the three others, forming two parcels of bars, lying in the ſame direction, and about three inches aſunder, which nearly was the length of the ſteel bar which was intended to be rendered magnetic. Things being thus diſpoſed, the ſteel bar was made quite red hot, and in that ſtate was placed between the magnetic bars; after which cold water was immediately poured upon it, which rendered it ſo hard as not to admit being filed: its magnetiſm was found to be conſiderably ſtrong, but by no means extraordinary. From repeated trials with ſteel bars of different ſizes, and by uſing a greater or leſs number of magnetic bars, I found, that ſhort ſteel bars acquire a proportionably greater degree of magnetiſm by this method, than thoſe which are longer; that the magnetiſm in the longer bars is not proportionably as ſtrong, principally becauſe the artificial magnets being placed at their extremities have very little power on thoſe parts of the pieces of ſteel which are nearer its center; and, laſtly, that when, in order to remedy the juſt mentioned impediment, more magnets are placed nearer the middle of the ſteel bar, then this piece of ſteel generally acquires many ſucceſſive magnetic poles.

Upon the whole it ſeems, that though this method alone be not ſufficient to communicate to ſteel bars an extraordinary degree of magnetiſm, yet it may be of great uſe in conſtructing large artificial magnets; for if thoſe bars, inſtead of being hardened in the uſual way by plunging them when red hot in water, be hardened whilſt ſtanding between powerful magnets, they will thereby acquire a conſiderable degree of magnetic power without any additional trouble to the workmen.

men. They may then be poliſhed, after which they may be rendered more ſtrongly magnetic by the uſual method of touching them with other magnetic bars; whereas it is a very laborious operation to render magnetic large bars of hardened ſteel from the very beginning, *viz.* when they have none of that power.

In the courſe of performing thoſe trials, I frequently obſerved, that the pieces of ſteel, whilſt they were red hot, ſeemed not to be attracted by the magnets; ſo that the leaſt ſhock, and even the pouring of the water, could remove them from the proper ſituation, which rather ſurpriſed me; becauſe it has been aſſerted, by ſome authors, that the magnet attracts red-hot iron as well as cold. KIRCHER eſpecially ſays, that he tried the experiment *, and found that a piece of iron heated ſo as to be hardly diſcerned from a burning coal, was attracted by the magnet as eaſily as when cold; and he even aſſigns a reaſon why the power of a magnet is deſtroyed by a great degree of heat; whereas the red heating of the iron will not prevent its being attracted by the magnet. The reaſon he gives is, that the fire corrupts and calcines the magnet, but purifies the iron. The following experiments were made in order to aſcertain this matter.

I kept a piece of ſteel in the fire till it was quite red hot, and in that ſtate preſented the magnet to it, ſo as to touch it repeatedly in various places; but no ſign of attraction could be perceived before the redneſs diſappeared. I mean, however, ſuch redneſs as may be evidently ſeen in the clear daylight; for, as was ſhewn by other experiments, when the magnet begins to attract the heated iron, the redneſs of the latter can ſtill be ſeen in the dark.

* De Magnete, lib. I. p. II. theorem xxxi.

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Having repeated the experiment with different pieces of iron and of ſteel, the reſult was conſtantly the ſame, *viz.* whilſt the iron or ſteel remained quite red hot or white hot, the magnet did not attract it; but the attraction began when that degree of redneſs, which is clearly perceivable in the daylight, began to diſappear; and it was as ſtrong as ever when the iron was cooled a little more than when the redneſs quite diſappeared in the dark. In regard to this limit or maximum of attraction, I think I have obſerved, as well as the nature of the experiments would permit, a difference between ſteel and iron, which is, that in the ſteel the maximum of attraction follows the diſappearance of the red heat ſooner than in iron.

This experiment is ſubject to two ſources of miſtake, which perhaps miſled Father KIRCHER, and which are neceſſary to be mentioned for the ſake of others who wiſh to repeat this experiment. The firſt is, that when a piece of iron of no great extent is red-hot, or even white hot, in one place, and below a red heat in other parts, the magnet will frequently attract it, though the red-hot ſide be preſented to it. The ſecond cauſe of miſtake is, that when a ſmall piece of iron or ſteel, as a common ſewing needle, is made red-hot, and is then preſented to the magnet, if the magnet touch it, that contact cools it inſtantly below the neceſſary degree of heat, and of courſe the attraction takes place. It is owing to this laſt cauſe that I have not yet been able to aſcertain, whether the attraction between the magnet and the iron be quite annihilated, or only diminiſhed to a great degree, by rendering the iron red or white hot; ſo that I can only ſay with certainty, that a magnet will not attract a certain piece of iron red hot or white hot; whereas it will attract another piece of iron at leaſt fifty times bigger, if it be cold or below a red heat.

To try this property in a different and more convincing way, I heated a large iron nail till it was white hot, and in that ſtate placed it upon an earthen ſupport near one pole of the magnetic needle, ſo as to lie not in the ſame direction, but on one ſide of it. Then, looking attentively on the graduated circle of the compaſs, I obſerved, that the needle was not in the leaſt moved from its natural ſituation, whilſt the nail remained red-hot; but, as ſoon as the redneſs began to diſappear, the needle advanced towards the nail, and a few ſeconds after the needle pointed directly towards it.

I tried whether in this experiment any difference was occaſioned by the magnets being natural or artificial; but, as it might be expected, there was none.

In purſuance of thoſe magnetic experiments wherein heat is concerned, I tried the effects which took place when the magnet was heated; but as the diminution of its power by heating, and an increaſe of it by cooling, were obſerved and deſcribed by the late Mr. CANTON, I ſhall only add a circumſtance, which may perhaps be new. It is that an artificial magnet, after having had its power diminiſhed by heating, does not recover it intirely again by cooling; having conſtantly found, that the magnets which had been heated, after cooling would never hold as great a weight of iron as they did before. The heat to which thoſe magnets were expoſed never exceeded that of boiling water. This was rendered more evident by the following experiment:

A magnetic bar was placed in an earthen veſſel at ſome diſtance from the ſouth pole of the needle of a very good compaſs; by the action of which magnet that end of the needle was drawn ſeveral degrees from the magnetic meridian, or the direction in which it ſtood before. In this ſituation of the

apparatus

apparatus boiling water was poured into the veſſel wherein the magnet ſtood, in conſequence of which the needle went back two degrees and a half. Some time after, when the water was quite cold, the needle was found nearer to the magnet, but not ſo near as it ſtood before the hot water was poured into the veſſel.

Next to the effects of heat, I was deſirous of trying what could be effected by decompoſing the iron; and for this purpoſe an earthen veſſel, containing about two ounces of iron filings, was placed near the ſouth end of the needle of the compaſs, by which the needle was drawn a little out of its natural direction. Having marked where the needle now ſtood, ſome water firſt, and then ſome vitriolic acid, were poured upon the filings, which occaſioned a briſk effervescence, and a copious production of inflammable air; but ſoon after the beginning of the effervescence, I was ſurpriſed to obſerve, that the needle came nearer to the veſſel, ſhewing that the attraction between the needle and the filings had been increaſed by the action of the vitriolic acid upon the latter, which is contrary to what could have been expected; for if we conſider that the power of a magnet is diminiſhed by heat, and that red-hot iron has either no attraction at all, or an exceedingly ſmall degree of it, towards the magnet, we might have concluded, that the action of the vitriolic acid upon iron would immediately diminiſh its attraction, beſides the other ſtrong reaſon ariſing from the dephlogiſtication of the iron occaſioned by the effervescence; and in fact ſome time after, when the violence of the effervescence, and of courſe the production of inflammable air, begins to abate, the attraction between the needle and the filings begins likewiſe to diminiſh; and at laſt, when the effervescence is hardly perceptible, the needle is found to

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ſtand farther from the veſſel containing the filings, &c. than it ſtood before the vitriolic acid was added, which diminution of attraction is certainly owing to the loſs of phlogiſton; it being well known, that iron is leſs and leſs attracted by the magnet in proportion as it approaches nearer to the calcined ſtate. Here follows the particular account of the above-mentioned experiment.

The ſouth end of the needle coincided with the 285° 15′ on the divided circle. When the pot with the iron filings was placed on one ſide of it, the needle pointed to 286° 15′, being drawn juſt one degree nearer. Having added the diluted vitriolic acid to the filings, the needle came nearer, and ſtood at 286° 45′. Ten minutes after the beginning of the effervescence it ſtood at 286° 35′, having receded a little; and a few minutes after this obſervation it ſtood at 286° 30′. An additional quantity of diluted vitriolic acid was now added, which increaſed the effervescence conſiderably; and on obſerving it a ſhort time after, the needle was found at the ſame point at which it ſtood before, from which time it began to go back very gradually; ſo that about three hours after it ſtood at 285° 50′, *viz.* farther from the effervescing mixture than it ſtood before any vitriolic acid was poured upon the iron filings.

As a ſingle experiment ought not to be depended upon when an error may be occaſioned by many concurring circumſtances, I repeated this experiment with great precaution, taking care that nothing could ſhake the needle, or the reſt of the apparatus; but the reſult was nearly the ſame, the attraction between the iron filings and the needle being increaſed by the action of the vitriolic acid.

In order to aſcertain that this effect was not owing to the heat generated by the effervescence, the pot, with ſome iron

filings, was placed near the magnetic needle, as before; then ſome boiling water was poured upon the filings, which heated them much more than the diluted vitriolic acid could have done; but the magnetic needle was not moved in the leaſt from its original ſituation.

The ſuſpicion which occurred next was, whether the effervescence might not agitate the iron filings ſo as to bring a greater number of them to that ſide of the veſſel, which ſtands contiguous to the magnetic needle. In order to obviate this objection, the experiment was repeated with a ſingle piece of iron inſtead of the filings; but as this piece of iron preſented a very ſmall ſurface to the diluted acid, the effervescence was very weak, and of courſe the magnetic needle did not move when the acid was poured into the pot. At laſt, in order to remedy this ſecond inconvenience, ariſing from the want of ſurface, I uſed a long piece of ſmall ſteel wire, which was twiſted in various directions, ſo as to be admitted into the pot; in which caſe the metal preſented a large ſurface to the acid, and it was not ſubject to be moved by the effervescence. The reſult was ſimilar to that of the firſt experiment, *viz.* the attraction was increaſed by the action of the acid on the wire; and here follows the particular account of the experiment.

About ſix yards of clean ſteel wire, ſomewhat leſs than one-fiftieth of an inch in diameter, being twiſted in various directions, was put into an earthen veſſel, which was placed near the ſouth end of the magnetic needle, which in conſequence of that was drawn from its natural ſituation, *viz.* from 281° to 280°. After adding the diluted vitriolic acid, a ſtrong effervescence commenced, and the needle came to 279° 47′. About five minutes after, it ſtood at 279° 35′. Five minutes after this, it ſtood at 279° 30′. And a little after this obſervation, it appeared

peared to be even ſomewhat nearer to the pot than the above-mentioned point. The experiment was then diſcontinued, and on removing the pot, the needle went back to its original ſituation, *viz.* 281°; which ſhewed, that its alteration during the proceſs was occaſioned by the action of the acid on the iron, and not by any extraneous cauſe.

On examination the wire was found only blackened on its ſurface, but not nearly conſumed; I had therefore the curioſity of trying the ſame wire again, and accordingly it was placed in the ſame veſſel near the magnetic needle, which attracted the latter from its original ſituation 281° to 280°. After adding the acid, the needle came nearer, as in the preceding experiment; and a ſhort time after it ſtood at 279° 30′, at which time the pot was removed, there being no occaſion to continue the experiment any longer.

On pouring the liquor out of the pot, the wire did not appear to be much waſted. The pot was then replaced near the needle, ſo as to attract it a little nearer as before; but on pouring boiling water upon the wire, a pretty briſk effervescence took place, and the needle was in conſequence attracted ſtill nearer. This experiment ſhewed, that though the diluted acid had been poured out, yet there remained a quantity of it adhering to the wire, which was ſufficient to renew the effervescence, when aſſiſted by the heat of the boiling water.

Upon the whole, it appears, that the action of vitriolic acid upon iron or ſteel increaſes their attraction towards the magnet; that this increaſe of attraction has a limit, after which it begins to decreaſe; and that this limit ſeems to come ſooner when iron than when ſteel is uſed; but, however, in reſpect to this laſt particular I am not yet quite certain, ſince, in the

experiments hitherto made, the variety in the ſhape or bulk of the iron or ſteel may have occaſioned a conſiderable difference.

After the reſult of thoſe experiments, it was natural to examine the effect which other acids might have on iron and ſteel; therefore the above-mentioned experiment of the ſteel wire was repeated with nitrous inſtead of vitriolic acid; the reſult of which was that the attraction between the magnetic needle and the wire was increaſed, but not ſo much as when vitriolic acid had been uſed. Here follows the particular account of the experiment.

About ſix yards of clean ſteel wire, near one-fiftieth of an inch in diameter, being twiſted in various directions, was placed in the uſual veſſel near the ſouth end of the needle; in conſequence of which the needle was attracted from its natural ſituation, *viz.* from 283° 32′ to 282° 50′. About two ounces of water were then poured over the wire, and immediately after, near one ounce of nitrous acid was added, which produced an effervescence: the magnetic needle, however, hardly moved from its former ſituation; but in about a minute's time, the effervescence being increaſed very much, the needle came to 282° 42′. About a quarter of an hour after, the violence of the effervescence abated a little, and the needle went back again to 282° 50′. A ſhort time after, it ſtood at 283° 2′. At laſt, when about half an hour had elapſed ſince the beginning of the operation, the effervescence was hardly perceivable, the liquor was become red like the colour of red ochre, and the needle ſtood at 283° 15′, viz. farther from the veſſel than it did before the acid was added to the wire.

It appears, therefore, that the effervescence occaſioned by the nitrous acid produced a ſimilar effect, though not in ſo great a degree as the vitriolic. The maximum of the attraction ſeems

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to come ſooner when nitrous than when vitriolic acid is uſed, after which limit the attraction decreaſes much faſter in the former than in the latter caſe, which is evidently owing to the metal being more eaſily dephlogiſticated and diſſolved by the nitrous than by the vitriolic acid.

The marine acid was tried next; but, notwithſtanding all the precautions I could take, it always occaſioned a very weak effervescence, and the needle was not in the leaſt affected by it.

A ſtrong effervescence ſeems to be neceſſary to increaſe the attraction between the magnet and iron or ſteel; for when I tried the experiment by putting a ſmall quantity of acid into the veſſel, the effervescence was exceedingly weak; the magnetic needle was not at firſt affected by it, but ſeveral hours after it ſtood a little farther from the pot, which was evidently owing to the dephlogiſtication of the iron.

After obſerving the action of acids on iron, I next tried to decompoſe that metal by means of fire, to obſerve what effect would take place with reſpect to its magnetiſm. For this purpoſe two ounces of iron filings were mixed with an ounce of flowers of ſulphur and an ounce of nitre. This mixture was put into a ſmall and ſhallow earthen veſſel, and was placed near the ſouth extremity of the magnetic needle, which attracted the needle nearer than its natural ſituation by about one degree and a half. A pane of glaſs was interpoſed between the magnetic needle and the veſſel. Things being thus diſpoſed, the mixture was fired, and it burned rather ſlowly, the fire ſometimes going out, ſo as to require being fired again; till at laſt it would burn no longer. During this combuſtion the needle was once attracted ſomewhat nearer; but its motion that way was ſo little, that I could not be quite certain of it. This happened not long after the fire was firſt communicated to the mixture,

after

after which the needle generally vibrated backwards and forwards, but upon the whole it gradually receded.

On repeating the experiment with a larger quantity of the mixture, and alſo with different proportions of ingredients, I could not obſerve any particular attraction. The needle vibrated rather more than before, but gradually receded; ſo that at laſt it ſtood farther from the veſſel than it did before the mixture was ſet on fire, though not quite in its natural direction.

The vibration of the needle in this experiment, or its waving motion, was probably owing to the irregular burning of the mixture, and perhaps to the heating of one part of it whilſt the other was burning. The gradual receding of the needle was certainly owing to the dephlogiſtication of the iron.

After having thus related the reſult of experiments, I ſhall now beg leave to add a few thoughts concerning the application of thoſe obſervations towards accounting for the variation of the magnetic needle.

This wonderful phænomenon has, ſince it was firſt diſcovered, employed the thoughts of very able philoſophers; many hypotheſes having been offered, not only for its explanation, but even to foretel the future variations in various parts of the world. I need not detain this learned Society with a particular hiſtory of thoſe hypotheſes, but ſhall only obſerve in general, that neither their predictions have anſwered, nor any of them was founded upon evident principles. The ſuppoſition of a large magnet being incloſed within the body of the earth, and of its relatively moving with reſpect to the outward ſhell or cruſt; the ſuppoſition of there being four moveable magnetic poles within the earth; the hypotheſis of a magnetic power, partly within and partly without the ſurface of the earth; together with ſeveral other hypotheſes on the ſame ſubject, are not only

only unwarranted by actual experiments, but do neither seem analogous to the other operations of nature. The late ingenious Mr. CANTON, Member of this Society, was the first, who endeavoured to account for the daily variation of the magnetic needle by the heating and cooling of the magnetic bodies in different parts of the earth's surface; which was in consequence of his having first observed, that the action of the magnet on the needle was diminished by heating, and increased by cooling *.

Following Mr. CANTON's judicious method of deriving the explanation of natural appearances from properties actually proved by experiments, I think, that the increase and diminution of magnetic attraction by heating and cooling of the magnet, as observed by Mr. CANTON, together with the result of the experiments recited in this Paper, seem fully sufficient to account for the general variation of the needle.

If we collect under one point of view all the causes hitherto ascertained, which can increase or diminish the attraction between magnetic bodies, we shall find, that the attraction between the magnet and iron, or between magnet and magnet, is increased by cooling, by a regeneration of iron or phlogistication of its calx, and within certain limitations by the action of acids upon the iron; that this attraction is diminished by heating, and by the decompofition of iron; and, lastly, that it is probably annihilated by a very great degree of heat.

These truths being premised, it must be considered, first, that, according to innumerable observations and daily experience, the body of the earth contains almost every where ferruginous bodies in various states and bulks; secondly, that the magnetic needle must be attracted by all those bodies, and its

* Philosophical Transactions, vol. LI. p. 398.

situation

ſituation or direction muſt be determined by all thoſe attractions conſidered together, *viz.* from their common center of action; thirdly, that by removing or altering the degrees of attraction of ſome of thoſe bodies which are ſituate on one ſide of the magnetic meridian, more than of thoſe ſituated on the other ſide, the above-mentioned common center of attractions, and, of courſe, the direction of the magnetic needle muſt be altered, which in fact is the variation of the needle; and, laſtly, that this alteration in the attractions of ſome of the ferruginous bodies in the earth muſt undoubtedly take place, it being occaſioned by the parts of the earth being irregularly heated and cooled, by the action of volcanoes which decompoſe or otherwiſe alter large maſſes of ferruginous ſubſtance, by earthquakes which remove ferruginous bodies from their original places, and we may add alſo by the *aurora borealis*; for though we are as yet ignorant of the cauſe of that ſurpriſing phænomenon, it is however certain, that the magnetic needle has been frequently diſturbed when the *aurora borealis* appeared very ſtrong.

The magnetic needle, therefore, being neceſſarily affected by thoſe cauſes, it ſeems unneceſſary to have recourſe to other hypothetical cauſes which are not eſtabliſhed on actual experience.

In order to exemplify this explanation of the variation in a familiar manner, I made the following experiment, with the account of which I ſhall conclude this Paper. Four earthen veſſels were diſpoſed round the magnetic needle, two near its ſouth, and the other two near its north pole, but not at equal diſtances. In one of thoſe veſſels there was placed a natural magnet; the ſecond contained ſeveral ſmall bits of magnetic ſteel mixed with earth; and in each of the other two there were

put

put about four ounces of iron filings. Things being thus disposed, and left undisturbed for about half an hour, the needle remained unaltered. Then the pieces of magnetic steel and earth were stirred with a stick, in consequence of which the needle was agitated. After this, some diluted vitriolic acid was poured upon the filings in one of the vessels, the action of which attracted the needle that way; but whilst the needle remained in that situation, some diluted vitriolic acid was poured upon the iron filings in the other vessel, which stood on the other side, in consequence of which the needle went back again towards its former direction. Whilst the effervescences were going on in the two vessels, the magnet in the first vessel was heated by means of boiling water, which occasioned another alteration in the direction of the magnetic needle; and thus, by altering the state of the ferruginous substances in the vessels, the needle's direction was altered, in evident imitation of the natural variation.

T. CAVALLO.

IV. *Description of a new Electrometer. In a Letter from the Rev.* Abraham Bennet, *M. A. to the Rev.* Joseph Priestley, *LL.D. F.R.S.*

Read December 7, 1786.

REV. SIR, Wirksworth, Sept. 14, 1786.

I SEND you a description of my electrometer, which, having the honour of your approbation, may be communicated to the Royal Society. (See Tab. II. fig. 1. and 2. which represent two sections of the instrument.)

It consists of two slips of leaf gold, *aa*, suspended in a glass *b*. The foot *c* may be made of wood or metal; the cap *d* of metal. The cap is made flat on the top, that plates, books, evaporating water, or other things to be electrified, may be conveniently placed upon it. The cap is about an inch wider in diameter than the glass, and its rim about three-quarters of an inch broad, which hangs parallel to the glass, to turn off the rain and keep it sufficiently insulated. Within this is another circular rim, about half as broad as the other, which is lined with silk or velvet, and fits close upon the outside of the glass; thus the cap fits well, and may be easily taken off to repair any accident happening to the leaf gold. Within this rim is a tin tube *e*, hanging from the center of the cap, somewhat longer than the depth of the inner rim. In the tube a small peg *f* is placed, and may be occasionally taken out. To the

peg,

peg, which is made round at one end and flat at the other, two ſlips of leaf gold are faſtened with paſte, gum-water, or varniſh. Theſe ſlips, ſuſpended by the peg, and that in the tube faſt to the center of the cap, hang in the middle of the glaſs, about three inches long, and a quarter of an inch broad. In one ſide of the cap there is a ſmall tube *g*, to place wires in. It is evident, that without the glaſs the leaf gold would be ſo agitated by the leaſt motion of the air, that it would be uſeleſs; and if the electricity ſhould be communicated to the ſurface of the glaſs, it would interfere with the repulſion of the leaf gold; therefore two long pieces *b b* of tin-foil are faſtened with varniſh on oppoſite ſides of the *internal* ſurface of the glaſs, where the leaf gold may be expected to ſtrike, and in connexion with the foot. The upper end of the glaſs is covered and lined with ſealing-wax as low as the outermoſt rim, to make its inſulation more perfect. Tab. III. fig. 1. repreſents the inſtrument joined together, and ready for uſe.

The following experiments will ſhew the ſenſibility of this inſtrument. See Tab. IV.

1ſt, Powdered chalk was put into a pair of bellows, and blown upon the cap, which electrified it poſitively when the cap was about the diſtance of ſix inches from the nozzle of the bellows; but the ſame ſtream of powdered chalk electrified it negatively at the diſtance of three feet, as repreſented in fig. 2. and 3. In this experiment there is a change of electricity from poſitive to negative, by the diſperſion or wider diffuſion of the powder in the air. It is alſo changed by placing a bunch of fine wire, ſilk, or feathers, in the nozzle of the bellows, and is wholly negative when blown from a pair of bellows without their iron pipe, ſo as to come out in a larger ſtream; this laſt experiment did not anſwer in dry weather ſo well as in wet.

The poſitive electricity of the chalk, thus blown, is communicated becauſe part of the powder ſticks to the cap; but the negative is not communicated, the leaf gold collapſing as ſoon as the cloud of chalk is diſperſed.

2dly, A piece of chalk drawn over a bruſh, or powdered chalk put into the bruſh, and projected upon the cap, electrifies it negatively; but its electricity is not communicated. Fig. 4.

3dly, Powdered chalk blown with the mouth or bellows from a metal plate placed upon the cap, electrifies it permanently poſitive. Fig. 5. Or if the chalk is blown from the plate, either inſulated or not, ſo that the powder may paſs over the cap, if not too far off, it is alſo poſitive. Or if a bruſh is placed upon the cap, and a piece of chalk drawn over it, when the hand is withdrawn the leaf gold gradually opens with poſitive electricity as the cloud of chalk diſperſes.

4thly, Powdered chalk falling, from one plate, to another placed upon the inſtrument, electrifies it negatively. Fig. 6.

Other methods of producing electricity with chalk and other powders have been tried; as projecting chalk from a gooſe wing, chalking the edges of books and clapping the book ſuddenly together, alſo ſifting the powder upon the cap; all which electrified it negatively: but the inſtrument being placed in a duſty road, and the duſt ſtruck up with a ſtick near it, electrified it poſitively. Breaking the glaſs tear upon a book electrified it negatively, probably by friction in the act of ſhivering, for when broken in water it did not electrify it.

Wheat flour, and red lead, are ſtrongly negative in all caſes where the chalk is poſitive. The following powders were like chalk: red ochre and yellow, roſin, coal aſhes, powdered crocus metallorum, aurum moſaicum, black-lead, lampblack (which was only ſenſible in the two firſt methods), powdered quick-

quick-lime, umber, lapis calaminaris, Spaniſh brown, powdered ſulphur, flowers of ſulphur, iron filings, ruſt of iron, ſand. Roſin and chalk, ſeparately alike, were changed by mixture; this was often tried in dry weather, but did not ſucceed in damp: white lead alſo ſometimes produced poſitive, and ſometimes negative, when blown from a plate.

If a metal cup be placed upon the cap, with a red-hot coal in it, a ſpoonful of water thrown in electrifies the cup negatively; and if a bent wire be placed in the cap, with a piece of paper faſtened to it to increaſe its ſurface, the poſitive electricity of the aſcending vapour may be tried by introducing the paper into it. Perhaps the electrification of fogs and rain is well illuſtrated by pouring water through an inſulated cullender, containing hot coals, where the aſcending vapour is poſitive, and falling drops negative. Fig. 7.

The ſenſibility of this electrometer may be conſiderably increaſed by placing a candle upon the cap. By this means a cloud of chalk, which only juſt opens the leaf gold, will cauſe it to ſtrike the ſides for a long time together; and the electricity, which was not before communicated, now paſſes into the electrometer, cauſing the leaf gold to repel, after it is carried away. Even ſealing-wax by this means communicates its fire at the diſtance of twelve inches at leaſt, which it would ſcarcely otherwiſe do by rubbing upon the cap.

A cloud of chalk or wheat flour may be made in one room, and the electrometer, with its candle, be afterwards leiſurely brought from another room, and the cloud will electrify it before it comes very near. The air of a room, adjoining to that wherein the electrical machine was uſed, was very ſenſibly electrified, which was perceived by carrying the inſtrument through it with its candle. Fig. 8.

In

In very clear weather, when no clouds were viſible, the electrometer has been often applied to the inſulated ſtring of kites without metal, and their poſitive electricity cauſed the leaf gold to ſtrike the ſides; but when a kite was raiſed in cloudy weather, with a wire in the ſtring, and when it gave ſparks about a quarter of an inch long, the electricity was ſenſible by the electrometer at the diſtance of ten yards or more from the ſtring; but, when placed at the diſtance of ſix feet, the leaf gold continued to ſtrike the ſides of the electrometer, for more than an hour together, with a velocity increaſing and decreaſing with the denſity or diſtance of the unequal clouds which paſſed over.

Sometimes the electricity of an approaching cloud has been ſenſible without a kite, though in a very unfavourable ſituation for it, being in a town ſurrounded with hills, and where buildings encompaſſed the wall on which the electrometer was placed. A thunder cloud paſſing over, cauſed the leaf gold to ſtrike the ſides of the glaſs very quick at each flaſh of lightning.

No ſenſible electricity is produced by blowing pure air, projecting water, by ſmoke, flame, or exploſions of gunpowder.

A book was placed upon the cap, and ſtruck with ſilk, linen, woollen, cotton, parchment, and paper, all which produced negative repulſion; but when the other ſide of the book was ſtruck with ſilk, it became poſitive; this ſide, ſtruck at right angles with the former, was again negative; and, by continuing the ſtrokes which produced poſitive, it changed to negative, for a little while; and, by ſtopping again, became poſitive. No other book would do the ſame, though the ſides were ſcraped unchalked, upon a ſuppoſition that altering the ſurface would produce it. At laſt, one ſide of a book was moiſtened, which changed it; ſo I concluded, that one edge

of the book had lain in a damp place; which conjecture was farther confirmed by all the books becoming positive in damp weather, and one of them being dried at the fire again became negative.

When the cap is approached with excited sealing-wax, the leaf gold may be made to strike the sides of the glass more than twelve times; and as the sealing wax recedes, it strikes nearly as often; but, if it approaches much quicker than it recedes, the second number will sometimes be greater.

The quantity of electricity necessary to cause a repulsion of the leaf gold is so small, that the sharpest point or edges do not draw it off without touching; hence it is unnecessary to avoid points or edges in the construction of this instrument.

I am, &c.

ABRAHAM BENNET.

V. *Appendix to the Deſcription of a new Electrometer. In a Letter from the Rev.* Abraham Bennet, *M. A. to* Charles Blagden, *M. D. Sec. R. S.*

Read December 21, 1786.

SIR, Wirkſworth, Dec. 18, 1786.

THE following deſcription of my Electrometer, connected with M. VOLTA's Condenſer, waits on the approbation of the Royal Society, to be inſerted in the Philoſophical Tranſactions, as an Appendix to my Paper, communicated by Dr. PRIESTLEY.

The metallic cap of the electrometer is for this purpoſe to be ground and poliſhed flat and ſmooth, to fit a piece of marble alſo poliſhed on both ſides, and varniſhed. In the ſide of the marble is to be fixed a glaſs or baked-wood handle. Laſtly, on the top is a ſmaller metallic plate, furniſhed alſo with an inſulating handle. The whole conſtruction will be underſtood by the annexed drawing (ſee Table III. fig. 2.). When a ſmall charge of electricity is communicated to the metal at A, whilſt the marble B is touched, the ſingle condenſer is charged, and its electricity (if in ſufficient quantity) will be ſenſible when the marble is lifted up by the handle C; but, if not now ſenſible, touch the ſmall plate D, whilſt thus lifted up from the cap, and then lift it off the marble by its inſulating handle, and preſenting

ſenting it to the cap of the electrometer (if not ſtill in too ſmall a quantity) it will cauſe a divergence of the leaf gold, by an electricity of the ſame kind with that which was communicated to the cap, and in which cap a ſmall quantity of electricity remains. Thus both the larger and ſmaller condenſers of M. VOLTA are connected with the electrometer, ſo as to be uſed in the moſt ſimple, expeditious, and convenient manner I can think of. Their amazing power of condenſing electricity is ſufficiently explained in M. VOLTA's Paper, before publiſhed in the Philoſophical Tranſactions.

To the experiments on blowing powders from a pair of bellows I have to add, that if the powder is blown at about the diſtance of three inches upon a plate which is moiſtened or oiled, its electricity is contrary to that produced by blowing upon a dry plate. This ſhews that the electricity of the ſtreams of powder iſſuing out of the bellows is only contrary to the more expanded part, becauſe it is within the influence of its atmoſphere; for when this is deſtroyed by the adheſion of the powder to the moiſtened plate, it is negative when the bellows are preſſed, as it was before poſitive when the more expanded part was negative.

I have alſo to add, that the experiments on evaporation of water may be tried with more eaſe and certainty of ſucceſs by heating the ſmall end of a tobacco pipe, and pouring water into the head, which, running down to the heated part, is ſuddenly expanded, and will ſhew its electricity when projected upon the cap of the electrometer, more ſenſibly than any other way I have tried. If the pipe be fixed in a cloven ſtick, and placed in the cap of one electrometer, whilſt the ſteam is projected upon another, it produces both electricities at once.

Spirit of wine and ether are electrified like water. Oil and vitriolic acid produced smoke without any change of electricity. In these experiments a long pipe is better than a short one.

I am, &c.

ABRAHAM BENNET.

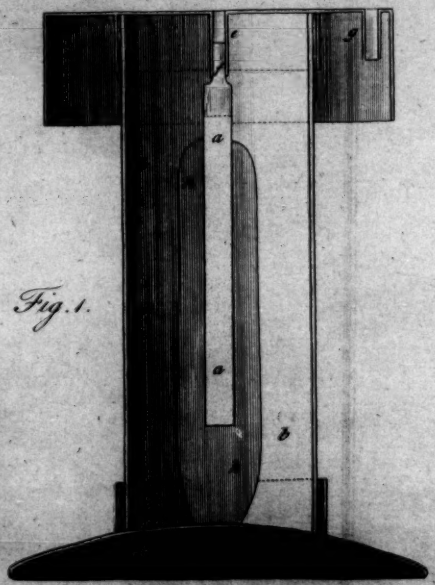


Fig. 1.

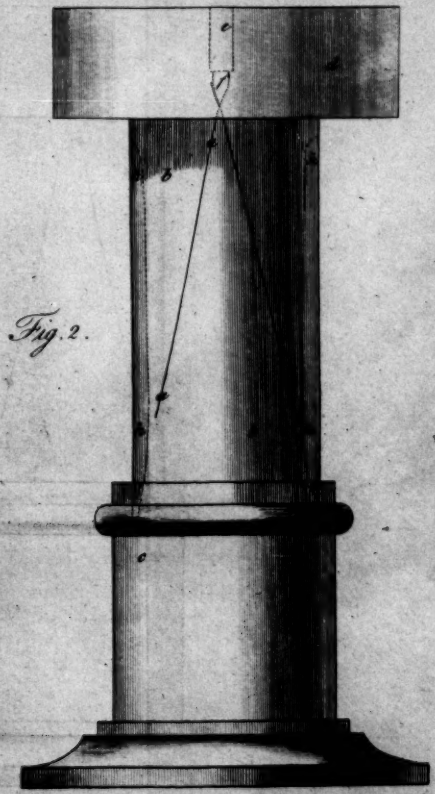


Fig. 2.

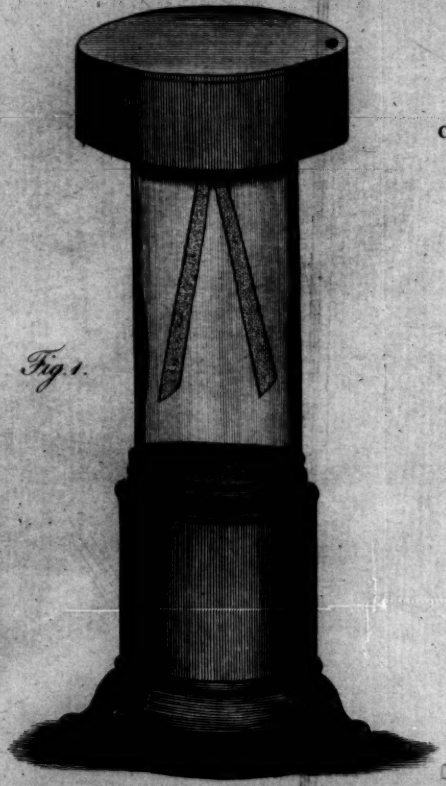


Fig. 1.

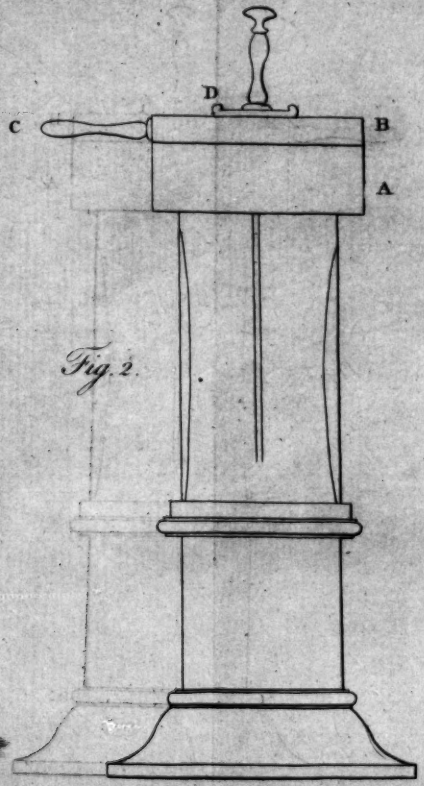


Fig. 2.

Fig. 2.

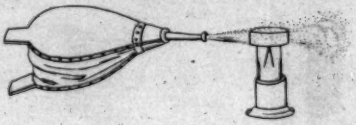


Fig. 3.

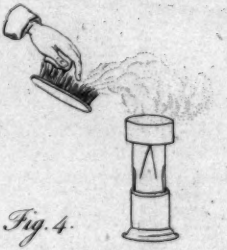


Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.

VI. *Some Account of an Earthquake felt in the Northern Part of* England. *In a Letter from* Samuel More, *Esq. to Sir* Joseph Banks, *Bart. P. R. S.*

Read December 7, 1786.

SIR, Caſtle-Head, Lancaſhire, Auguſt 22, 1786.

I TAKE the liberty of addreſſing this to you, to give you information, and furniſh you with all the particulars I have been able to collect, relative to a ſhock of an earthquake felt in this part of the kingdom on Friday, the 11th inſtant, about two o'clock in the morning; ſome ſay it was a little before that hour, others a little after, owing probably to the difference of the clocks.

I was at that time in the houſe of a miner at Aulſtone-Moor in Cumberland, but perceived no ſhock; nor do I believe it was felt in that neighbourhood, as no mention was made of it by any of the miners I converſed with during the courſe of the day; but, on my arrival at Penrith in the evening, every one there ſpoke of it as having been ſenſibly felt in that town. The next day, purſuing my journey, I was informed it had been felt along the banks of Ulſwater, in Patterdale, at Ambleſide, along the ſide of Winander Meer, and particularly at the houſe in the iſland on that lake, the property of Mr. CHRISTIAN. At the place where I am now writing, the Lady of the houſe, and ſome of the ſervants, were awakened by it, and deſcribe it as

ſhaking violently the beds, the chairs in the rooms, and the ſaſhes of the windows. At Cartmeal, a town about five miles from hence, it was alſo felt very ſeverely; and at the village of Carke, two miles from Cartmeal, a gentleman (Mr. FLETCHER STOCKDALE) tells me, he was awake ſome time before the ſhock; that he firſt heard a rumbling noiſe, like a carriage at a diſtance, and was conſidering what carriage could be moving at that hour, when he felt the ſhock. The noiſe continued ſome time after the ſhock was over; and he thinks the whole might laſt about four or five ſeconds, and it ſeemed to travel from the eaſt to the weſtward. Almoſt every body in the neighbourhood of Carke and Cartmeal were awakened by it, and ſome perſons much alarmed; but I do not find that, at any part where I have been, any damage has been done by it. At Lancaſter, about ten miles eaſt of Cartmeal, it was very plainly felt, particularly, as I am told, in the great tower of the Caſtle. It appears to have extended as far as Mancheſter, where it was ſlightly perceived.

Theſe are the particulars I have hitherto been able to collect relative to this earthquake, of which I doubt not you will have many accounts ſent you; but if theſe facts furniſh any thing not mentioned by your other correſpondents, it will afford great pleaſure to, Sir, &c.

SAMUEL MORE.

VII. *Determination of the Heliocentric Longitude of the descending Node of* Saturn. *By* Thomas Bugge, *Professor of Astronomy in the University of* Copenhagen; *communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read December 7, 1786.

THE culmination of Saturn was observed with a 6-feet achromatic transit-instrument, and the planet compared with o and π of Sagittarius, whose apparent right-ascensions in the middle of August 1784 were 282° 56′ 54″ and 284° 14′ 33″. The meridian altitude was observed with a 6-feet mural quadrant. The original observations are to be published in the second volume of my Astronomical Observations. From those are calculated the right-ascension and declination, the geocentric longitude and latitude, of Saturn, which are to be depended upon to 4 or 6 seconds. Those observed longitudes and latitudes are compared with the tables of Dr. HALLEY and of M. DE LA LANDE. In the errors of the tables + signifies that the longitude of the tables is less than the observed longitude; and the meaning of − is, that the calculated longitude is greater than the observed. It ought to be observed, that the heliocentric longitudes of Dr. HALLEY's Tables have been corrected for the perturbations after the principles of M. LAMBERT (Memoires de Berlin, pour 1783, p. 216. and Collection des Tables Astronomiques de Berlin, tom. II. p. 269.)

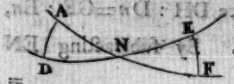
1784	♄ culmination, mean time at Copen.	♄ observed longitude.	♄ observed latitude.	The error of HALLEY in long.	The error of HALLEY in lat.	The error of M. DE LA LANDE. in long.	The error of M. DE LA LANDE. in lat.
	h. ′ ″	s. ° ′ ″	° ′ ″	′ ″	″	′ ″	″
July 12	12 3 1	9 20 34 48	0 3 35 B	+2 22	+32	−9 40	+33
20	11 29 9	9 19 59 39	0 2 59	+2 14	+38		
Aug. 1	10 38 25	9 19 9 22	0 1 44	+1 45	+27	−9 49	+28
8	10 9 0	9 18 42 56	0 1 2	+1 48	+21		
21	9 14 59	9 18 1 23	0 0 2	+1 35	+28		
27	8 50 19	9 17 46 19	0 0 29A	+1 26	−26	−9 35	−30
31	8 33 47	9 17 38 7	0 0 53	+1 19	−23		
Sept. 5	8 13 45	9 17 29 36	0 1 26	+1 20	−25	−9 25	−27
15	7 33 45	9 17 19 39	0 2 4	+1 18	−25		
Oct. 8	6 4 23	9 17 34 6	0 3 57	+1 22	−26	−8 48	−32

In order to reduce the observed geocentric longitude to the sun, or by observation to find the heliocentric longitude, it is required to know the angle at the planet $= p$. If this angle is calculated in the common way only by the tables, there will arise some difference, according to the different elements and the different constructions of the tables. Thus, at the time of Saturn's culmination, this angle is found the 12th of July, by the tables of Dr. HALLEY $= 0° 3' 13''$, and by the tables of M. DE LA LANDE $= 0° 2' 0''$; the 8th of August by HALLEY $= 2° 43' 35''$, and by M. DE LA LANDE $= 2° 42' 34''$; the 27th of August after Dr. HALLEY $= 4° 14' 15''$, and after M. DE LA LANDE $= 4° 13' 47''$. To avoid those differences, which often may alter the heliocentric longitude more than one or two minutes, the following method may be useful. The heliocentric longitude of the earth, calculated after the tables of M. MAYER, is to be depended upon to eight or ten seconds. From the heliocentric longitude of the earth, and from the observed geocentric longitude of the planet, corrected for the aberration and nutation, is deduced the angle at the earth $= t$,

or the distance between the sun and the planet seen from the earth. The dimensions of the elliptical orbit of the planet are so far ascertained, that the logarithms of the distance from the sun have not any material difference in the different tables. From the angle s, the distance of the earth from the sun; and the distance of the planet from the sun, the angle p is calculated to a sufficient degree of accuracy. Thus, the 12th of July, by the distances of Dr. HALLEY, $p = 0° 2' 59''$, and by the distances of M. DE LA LANDE $= 0° 2' 59''$; the 8th of August after Dr. HALLEY $p = 2° 43' 25''$, and after M. DE LA LANDE $p = 2° 43' 36''$; the 27th of August after Dr. HALLEY $p = 4° 14' 10''$, and after M. DE LA LANDE $p = 4° 14' 28''$. The difference very seldom will amount to 20 seconds, and is of no consequence in this matter. From the observed geocentric latitude of the angle at the sun $= s$, and the angle at the earth $= z$, the heliocentric latitude of the planet is found $=$

$$\frac{\text{tang. lat. geoc.} \times \text{sin. } z}{\text{sin. } s}$$

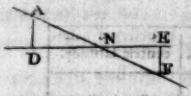
1784	Mean time at Copenhagen.	♄ observed heliocentric longitude.	♄ observed heliocentric latitude.
	h. ′ ″	s. ° ′ ″	° ′ ″
July 12	12 3 1	9 20 37 29	0 3 13 B
20	11 20 9	9 20 51 53	0 2 41
Aug. 1	10 38 85	9 21 13 17	0 1 34
8	10 9 0	9 21 26 2	0 0 56
21	9 14 59	9 21 49 27	0 0 2
27	8 50 19	9 22 0 12	0 0 27 A
31	8 33 47	9 22 7 32	0 0 59
Sept. 5	8 13 45	9 22 16 28	0 1 21
15	7 33 45	9 22 34 32	0 1 59
Oct. 8	6 4 23	9 23 16 15	0 3 35



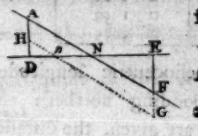
When two heliocentric longitudes, and the corresponding northern and southern latitude are given, the distance of

of the node from one of the longitudes or places may be found. Let DE be the ecliptic, AF the orbit of the planet, N the node, DE the difference between the two observed heliocentric longitudes $= a$, EF the ſouthern latitude $= \beta$, AD the northern latitude $= b$, NE the diſtance of the node from the heliocentric place at E, and corresponding to the ſouthern latitude $= x$. In the ſpherical triangles ADN and FEN, $\frac{\text{ſin. } (a-x)}{\text{tang. } b} = \text{cot. N} = \frac{\text{ſin. } x}{\text{tang. } \beta}$. By placing the value of ſin. $(a-x)$ in the equation $\frac{\text{ſin. } a \text{ . coſ. } x - \text{ſin. } x \text{ . coſ. } a}{\text{tang. } b} = \frac{\text{ſin. } x}{\text{tang. } \beta}$. By reſolving this equation $\frac{\text{ſin. } x}{\text{coſ. } x} = \text{tang. } x = \frac{\text{ſin. } a \text{ . tang. } \beta}{\text{tang. } b + \text{coſ. } a \text{ . tang. } \beta}$.

If a, b, and β, are very ſmall arcs, which commonly is the caſe with the planets, then ſin. $a = a$, tang. $\beta = \beta$, tang. $b = b$, and coſ. $a = 1$. Hence the ſpherical formula will be transformed into another $x = \frac{a\beta}{b+\beta}$. This formula belongs to plane geometry, and may beſides be thus demonſtrated. DN : NE = AD : EF. Hence DN + NE : NE = AD + EF : EF, and $\text{NE} = \frac{\text{DE} \times \text{EF}}{\text{AD} + \text{EF}}$. If



the difference of the longitudes do not exceed one degree, and the latitudes are not greater than ten minutes, the ſpherical and the rectilineal formula will agree to very few ſeconds. Small faults in the longitude will not very much alter the true place of the node; but very ſmall errors in the latitude are of great conſequence. Let the error in the ſouthern heliocentric latitude be FG = $+d$. The error in the northern latitude AH = $-d$. Hence DH : Dn = GE : En, and $\text{E}n = \frac{a(\beta+d)}{b+\beta}$. By ſubtracting EN



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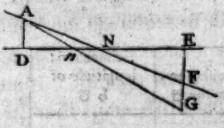
$= \frac{ad}{b+\beta}$, the error in the heliocentric longitude of the node, $Nn = \frac{ad}{b+\beta}$. If the fault in the southern latitude $= -d$, in the northern latitude $= +d$, the same formula is still true; but then $EN > En$, and the place of the erroneous node will be between E and N. In both cases the errors in the place of the node are directly as the errors in the latitudes.



Let us now suppose, that only the one latitude is erroneous $\pm d$. Then $Nn = \pm En \mp EN = a \times \left(\pm \frac{\beta \pm d}{b+\beta \pm d} \mp \frac{\beta}{b+\beta} \right) = \frac{abd}{(b+\beta)^2 \pm d(b+\beta)}$. In the case when the error in both latitudes is positive $= +d$, and $\beta > b$, or $\beta < b$, the resulting error in the place of the node $= \frac{ad(\mp b \pm \beta)}{(b+\beta)^2 + 2d(b+\beta)}$. In the case when the error in both latitudes is negative $-d$, and $\beta > b$, or $\beta < b$, then the error in the node $= \frac{ad(\mp b \pm \beta)}{(b+\beta)^2 - 2d(b+\beta)}$. In those two cases the error is less than in any of the former, and quite nothing when $b = \beta$. If the radius of the instrument, with which the meridian altitudes are observed, is given, the quantity of d is also given. In a mural quadrant of 6 or 8 feet $d = 5$ or 3 seconds. Take $a = 34' 3''$, $b = 56''$, $\beta = 27''$, $d = 5''$, and the error in the southern latitude $+ d$, in the northern $= -d$; then $Nn = \frac{10215''}{83} = 2' 12''$. Take now the error only in the southern latitude $= +d$; then $Nn = \frac{572040''}{7304} = 1' 18''$; in the case of $-d$; $Nn = \frac{572040''}{6474} = 1' 28''$. From hence it appears, that in comparing

two single observations, it scarce will be possible to avoid a fault of ± 2 minutes in the place of the node.

If the instrument is of a less force than a mural quadrant of 6 feet, and the possible faults in the altitudes greater, for example, 10 or 15 seconds, the resulting error in the place of the node may very easily be calculated; but the error in the node will be enormous, and the observations of no use for a nice astronomer.

Compared Observations 1784.	♄ heliocentric longitude on the last day.	♄ distance from the ☉.	Heliocentric longitude of ♄ ☊
	s. ° ′ ″	° ′ ″	s. ° ′ ″
July 12 with Sept. 15	9 22 34 32	0 44 38	9 21 49 54
July 12 — Oct. 8	9 23 16 15	1 23 41	9 21 52 36
July 20 — Sept. 15	9 22 34 32	0 43 37	9 21 50 55
July 20 — Oct. 8	9 23 16 15	1 22 33	9 21 53 42
Aug. 1 — Sept. 5	9 22 16 28	0 29 15	9 21 47 13
Aug. 1 — Sept. 15	9 22 34 32	0 45 23	9 21 49 9
Aug. 1 — Oct. 8	9 23 16 15	1 25 54	9 21 50 41
Aug. 8 — Aug. 27	9 22 0 12	0 11 7	9 21 49 5
Aug. 8 — Aug. 31	9 22 7 32	0 19 36	9 21 47 56
Aug. 21 — Aug. 27	9 22 0 12	0 10 0	9 21 50 12
Aug. 21 — Aug. 31	9 22 7 32	0 17 23	9 21 50 9
		Mean	9 21 50 8,5

This mean agrees pretty well with the observations on the 21st, 27th, and 31st of August, which are nearest the node, and most to be depended upon.

The 21st of August, at 9 h. 12′ 26″ true time at Copenhagen, the heliocentric longitude of Saturn = 9 s. 21° 40′ 27″, and the distance from the node = 4′. The 27th of August, at 8 h. 49′ 23″, the heliocentric longitude = 9 s. 22° 0′ 12″; therefore, in 5 days 23 h. 36′ 57″ Saturn has described an arc of 10′ 45″, and 10′ 45″ : 5d. 23 h. 36′ 57″ = 4′ : 1. Hence

Saturn has spent 9 h. 7′ 44″ in going through those 41″; and *Saturn's passage through the node happened August* 21, 1784, *at* 18 h. 20′ 10″, *and the heliocentric longitude of his descending node* = 9 s. 21° 50′ 8″,5. The errors in the place of the node are relative to the tables of Dr. HALLEY + 19′ 39″, to the tables of M. CASINI + 16′ 4″, and to the tables of M. DE LA LANDE + 1′ 31″.

In the foregoing computation of Saturn's heliocentric longitude from the tables of Dr. HALLEY, this longitude has been corrected for the perturbation after the principles of M. LAMBERT. Though the geocentric places, calculated in this manner, will agree still better with the observations than without those perturbations, nevertheless they are only empiric, and not founded upon the theory and principles of gravitation; I shall therefore conclude this Paper, by adding the faults in the heliocentric places of Saturn, calculated only and directly from the tables of Dr. HALLEY, which may be of some use to improve those valuable tables.

1784	♄ heliocentric longitude from Dr. HALLEY's tables.	Error in longitude.	♄ heliocentric latitude from Dr. HALLEY's tables.	Error in latitude.
	s. ° ′ ″	″	° ′ ″	″
July 12	9 20 27 40	+9 49	0 2 45 B	+28
20	9 20 42 3	+9 50	0 2 17	+24
Aug. 1	9 21 3 41	+9 36	0 1 10	+24
8	9 21 16 19	+9 43	0 0 37	+21
21	9 21 39 45	+9 42	0 0 24 A	−22
27	9 21 50 34	+9 38	0 0 53	−26
31	9 21 57 47	+9 45	0 1 11	−21
Sept. 5	9 22 6 48	+9 40	0 1 45	−24
15	9 22 24 50	+9 42	0 2 22	−23
Oct. 8	9 23 6 33	+9 44	0 4 1	−26

VIII. *Deſcription of a Set of Halo's and Parhelia, ſeen in the Year* 1771, *in* North-America. *By* Alexander Baxter, *Eſq.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 7, 1786.

EXTRACT from a journal kept in the upper countries of North-America. At Fort Glouceſter, on the river of Lake Superior, ſix miles above the falls of St. Mary's, and as much from the head of the river, where it iſſues from the Lake.

"January 22, 1771. Laſt night and to-day the froſt has been more ſevere than at any time this winter: I was hardly able, at mid-day, to keep my face to the wind uncovered, though the ſun ſhone very bright, and the ſky clear.

"In the morning the wind was eaſterly, which went about with the ſun to the ſouth and weſtward, returning to the eaſt in the evening; a very ſmall breeze.

"A little before two o'clock P.M. obſerved as follows. There was a very large circle or halo round the ſun (ſee Tab. V.) within which the ſky was thick and duſky, the reſt of the hemiſphere being clear; and, a little more than one-third way from the horizon to the zenith, was a beautifully enlightened circle, parallel to the horizon, which went quite round, till the two ends of it terminated in the circle that ſurrounded the ſun; where

where, at the points of interſection, they each formed a luminous appearance about the bulk of the ſun, and ſo like him when ſeen through a thick hazy ſky, that they might very eaſily have been taken for him. (Mock ſuns or parhelia). Directly oppoſite to the ſun was a luminous croſs, in the ſhape of a St. Andrew's Croſs, cutting at the point of interſection the horizontal circle, where was formed another mock ſun, like the other two mentioned above. The two lower limbs of the croſs appeared but faintly a little way below the circle, the two higher reached a good way above the circle towards the zenith very clear and bright. In this horizontal circle, directly half-way betwixt the ſun of the croſs and thoſe at the ends of the ſame circle, were other two mock ſuns, ſame kind and ſize, one on each ſide; ſo that in this horizontal circle were five mock ſuns, at equal diſtances from one another, and in the ſame line the real ſun, all at equal heights from the horizon. Beſides theſe meteors, there was, very near the zenith, but a little more towards the circle of the real ſun, a rainbow of very bright and beautiful colours, not an entire ſemi-circle, with the middle of the convex ſide turned towards the ſun, which lowered as the ſun deſcended.

" It was a little before two o'clock P.M. when I firſt obſerved this phænomenon, and it continued in all its beauty and luſtre till about half after two.

" The croſs went gradually off firſt; then the horizontal circle began to diſappear in parts, while in others it was viſible; then the three mock ſuns fartheſt from the ſun, the two in the ſun's circle continuing longeſt; the rainbow began to decreaſe after theſe; and, laſt of all, the ſun's circle, but it was obſervable at three o'clock, or after it.

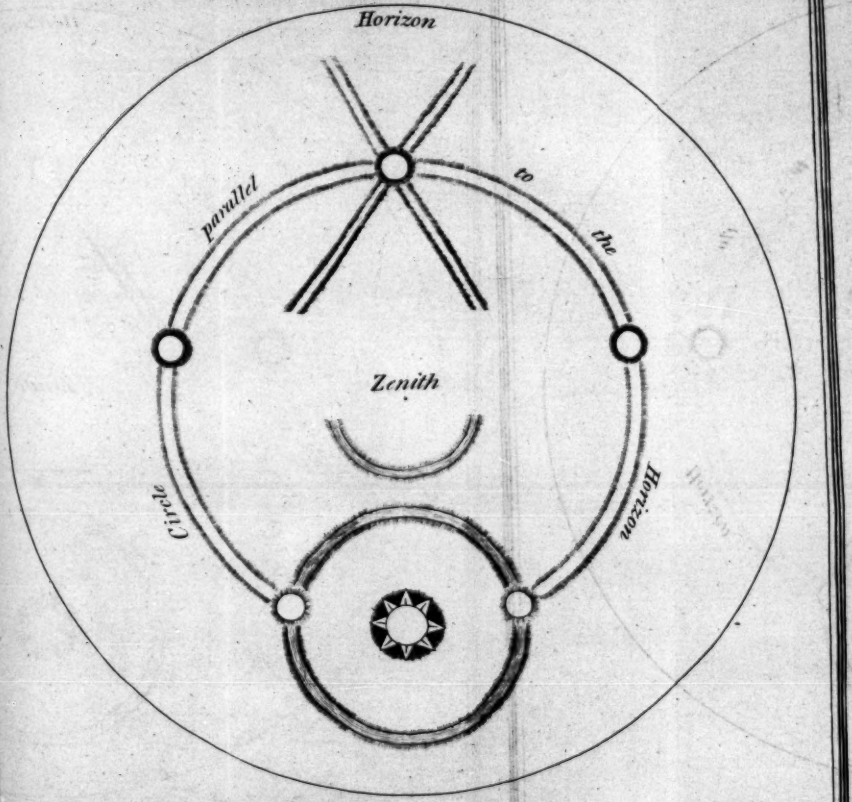
" The

"The weather continued fine, but the next day was a little softer."

I have subjoined a diagram of the phænomenon, in order to give a better idea of it than my feeble description, which I have copied literally, as it was set down in my journal at the time, and in a hurry.

ALEX. BAXTER.

Odiham, August 21, 1786.


Horizon
parallel
to
the
Zenith
Circle
Horizon

IX. *Observations of the Transit of* Mercury, May 4, 1786, *at* Dresden. *By M.* Köhler, *Inspector of the Mathematical Repository of the Elector of* Saxony; *communicated by the Count* de Brühl, *F. R. S.*

Read December 21, 1786.

Apparent time. h. ′ ″	
9 21 54	beginning of the planet's egress, doubtful.
9 25 23	complete egress, or last contact, very certain.

The telescope with which Mr. Köhler observed, was a 9-feet refractor of Dollond's, magnifying 104 times.

He has made a comparison of his observation with that of Alexander Aubert, Esq. from which he has inferred the longitude of Dresden in the following manner.

	Mean time. h. ′ ″
Exterior contact observed by Mr. Aubert at	8 27 5
Equation	+ 3 28,3
Difference of parallax	+ 0 13,4
Apparent time at Loam-pit Hill	8 30 46,7
Loam-pit Hill west of Greenwich	+ 0 5,4
Apparent time at Greenwich	8 30 52,1
Apparent time at Dresden	9 25 23,0
Longitude of Dresden east of Greenwich	54 30,9

X. *Obſervations of the Tranſit of* Mercury *at* St. Peterſburg. *In a Letter from M.* Rumovſki, *Aſtronomer in the Imperial Academy, to Mr.* J. H. de Magellan, *F. R. S.*

Read December 21, 1786.

J'AI l'honneur de vous communiquer mon obſervation ſur le dernier paſſage du Mercure, 1786, le $\frac{22 \text{ Avril}}{3 \text{ Mai}}$, t. aſtr.

	h.	′	″	
Le contact interne à l'entrée	17	2	19	t. v. Limbes ondoyans.
Le contact interne à la ſortie	22	26	55	. . . . bien terminés.

M. Tzernoi, qui m'a accompagné dans l'obſervation, n'a vu que le contact interne à la ſortie, 22 h. 27′ 7″.

J'omets les contacts externes: pour le premier, j'ai manqué de le voir; j'appercus le ☿ au ☉ à 16 h. 59′ 44″, quand la moitié à-peu-près en a été entrée: et pour le contact externe à la ſortie, j'ai douté juſqu'à la demi-minute.

Durant le paſſage, j'ai réuſſi à méſurer avec un micromètre objectif quelques diſtances des bords, et le diamètre de ☿; et je l'ai trouvé tantot de 7″,56, et tantot de 8″,64: en ſorte que je l'eſtime de 8″,2, ou plutôt de 8″,4.

Depuis peu j'eus le loiſir de calculer mes obſervations ſur les diſtances: et en ſuppoſant le demi-diamètre du ☉ 15′ 52″, et la parallaxe 8″,5, par 28 combinaiſons je trouve:

Là

La plus courte distance		11′	32″
	h.	′	″
Tems du milieu du passage	19	44	37 pour le merid. de Peters^b.
Tems de la conjonction	19	13	33 avec
La latitude géocentrique		11	43
	s.	°	′ ″
La longitude géocentrique	1	13	50 1
Et la longitude du Nœud	1	15	53 56

Pour reduire mon observation au centre, je trouve qu'il faut ôter du premier contact interne 1′ 41″, et ajouter 1′ 16″ au contact interne à la sortie: en sorte que suivant les élémens ci-dessus mentionnés, mon observation à l'entrée pèche en excès de 29″, et à la sortie de 4″ en defaut.

(Signed) RUMOVSKI.

XI. *An Account of the Strata observed in ſinking for Water at* Boſton, *in* Lincolnſhire. *By Mr.* James Limbird, *Surveyor to the Corporation; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 21, 1786.

ON the 7th day of May, 1783, GEORGE NAYLOR, of Louth, in the county of Lincoln, Well-borer, began to bore at the well in the Market-Place, Boſton; which had been ſunk and bored to the depth of 186 feet from the ſurface, in 1747, by THOMAS PARTRIDGE.

The well was made about 6 feet in diameter at the top, 5 feet in diameter at the bottom, and 27 feet deep, and the earth prevented from falling in by a circular frame of wood, which goes from the ſurface of the earth to the depth of 21 feet and 6 inches, and is there ſupported by brick-work, laid on a bed of light-coloured blue clay, which continues to the depth of 36 feet from the ſurface, where there is a bed of ſand and gravel about 18 inches thick, and under it the ſame ſort of blue clay as before, which continues to the depth of 48 feet from the ſurface. Below this there is a bed of dark-coloured ſtone, like ragſtone, about ſix inches thick, from under which GEORGE NAYLOR ſays, that a ſalt ſpring iſſues. Beneath this layer of ſtone, there is a bed of dark-blue clay, which continues to the depth of 75 feet from the ſurface, where is a bed of ſtone, of a lightiſh colour, about 6 inches thick, and under it

a bed of dark-blue clay, which continues to the depth of 114 feet from the ſurface, where there is a bed of ſtone, of a brightiſh colour, about 8 inches thick, and under it a bed of gravel, about 6 inches thick, where GEORGE NAYLOR ſays there is another ſalt ſpring. Under the gravel, there is a bed of dark-coloured clay, reſembling black-lead, which continues to the depth of 174 feet from the ſurface, when it changes to a chalky clay, intermixed with ſmall pebbles and flints, which continues about 3 inches, and then changes to the ſame kind of dark-coloured clay as before; in which, after boring to the depth of 186 feet from the ſurface, he came to the ſolid earth bored to, in 1747, by the above-mentioned THOMAS PARTRIDGE. After boring in the ſame kind of clay to the depth of 210 feet from the ſurface, it changes to a lighter-coloured one, which continues about 6 inches, and then changes dark again, and continues ſo to the depth of 342 feet from the ſurface, where there is a bed of ſhells and white-coloured earth, about half an inch thick, and under it a light-coloured earth like that at 210 feet from the ſurface, and under it a bed of dark-coloured clay. After continuing in that clay to the depth of 444 feet from the ſurface, GEORGE NAYLOR put down a tin pipe, 50 yards in length, and 2¼ inches in diameter within, to prevent the gravel and ſtones from falling down and obſtructing the rods; but being too weak for that purpoſe, it ſeparated into different lengths, and intirely prevented his boring, ſo that he was obliged to get the ſaid pipes up again, which took him 48 days; having got them up, and cleared the hole pretty well, he left off boring until he could procure ſome ſtronger pipes.

In July, 1784, he put down 21 pipes of caſt iron, which were caſt at Cheſterfield, in the county of Derby, each pipe

being 2¼ inches in diameter within, half an inch thick, and upon an average 6 feet and 1 inch in length; they were affixed together with boxes and ſcrews, and with a piece of ſoft leather between the top of each box and ſcrew, to prevent them from breaking; the uppermoſt pipe is faſtened to a plank, which lies upon the top of the brick-work.

At the diſtance of 447 feet from the ſurface there is a bed of dark-coloured earth mixed with chalk and gravel, which continues to the depth of 449 feet and 10 inches from the ſurface, where is a bed of dark-coloured earth without any chalk and with very little gravel, which continues to the depth of 454 feet and 7 inches from the ſurface; there it changes to a dark-coloured earth, mixed with chalk and gravel, which continues to the depth of 456 feet and 8 inches from the ſurface, and then changes to a dark-coloured earth without any chalk, and with very little gravel, which continues to the depth of 457 feet from the ſurface, and then changes of a lighter colour; and this continues to the depth of 462 feet and 4 inches from the ſurface, where it changes of a darker colour, and ſo continues to the depth of 470 feet and 3 inches from the ſurface. Here the ground changes to a dark-coloured earth, mixed with chalk and gravel, which continues to the depth of 470 feet and 7 inches from the ſurface, where he came to a bed of ſtone, like ragſtone, about 13 inches thick, which ground into powder with the wimble, and mixed with the earth. Under this bed of ſtone there is a dark-coloured earth, without any chalk, and with but little gravel, which continues to the depth of 472 feet from the ſurface, when it changes ſomething lighter, and continues ſo about 2 inches, where the earth appears to be mixed with chalk and gravel, and continues ſo for about

about 1 inch, when it changes to a black silt, having a great deal of light-coloured sand in it.

On September the 6th, 1785, GEORGE NAYLOR broke one of the screws belonging to his rods just above the top of the box, at the distance of between 92 and 93 yards from the surface; when the upper rod, having a circular head or ring 2 inches in diameter at the top, dropped down 40 yards through the iron pipes; which rods were got up again on the 15th day of September by a spring. After trying several instruments to get up the lower part of the rods, to no effect, on the 3d day of October following was contrived a spiral instrument, about 2 feet long, with a catch at the top of it, to take the bottom of the uppermost box of the rods that were down; but the top of the rods having fallen several inches from the perpendicular, prevented the instrument from taking them between the first and second boxes: therefore, the Surveyor to the Corporation and the above-mentioned GEORGE NAYLOR, on the 7th day of October, contrived a spiral instrument, about 2 feet long, without any catch at the top, which GEORGE NAYLOR put down about 10 yards below the upper box, and there taking hold of the rods, raked them up to the top, and by that means brought them perpendicular, when he left them, and on the 8th day of October put down the instrument invented before; by which he got hold of the rods a little below the top box, and brought them up. When the rods broke, GEORGE NAYLOR was boring in a dark-coloured silt, intermixed with chalk and gravel, at the distance of 474 feet from the surface, which continued to the depth of 475 feet and 5 inches, when it changed to dark-coloured wet silt without any chalk, in which GEORGE NAYLOR bored to the depth of 478 feet and 8¼ inches from the surface. Here he imagined,

by

by the eaſy turning of the wimble, that he had got into a ſpring of water, and gave over boring, to ſee if the water would riſe in the pipes; when, after keeping the water in the well below the top of the pipes for ſeveral days (by pumping), the water in the pipes was found to riſe about 5 feet *per* day upon an average; which only producing about 7 pints, it was ſuppoſed there was no ſpring of water bored into, but that the riſe of water in the pipes was occaſioned by the ſoccage only.

On Monday the 28th day of November, an iron bucket was made and affixed to the bottom of the rods, and let down the pipes, and filled with water at the depth of 85 yards from the ſurface; which water was ſalt, and of a reddiſh colour. The bucket was again let down, and filled at the depth of 156 yards from the ſurface; that water was more ſalt than the firſt, and much of the ſame colour.

The Committee appointed by the Corporation for ſuperintending the buſineſs of ſinking for water, having taken the whole of theſe circumſtances into their conſideration, and examined GEORGE NAYLOR, who did not account, in a manner ſatisfactory to them, for the ſlow progreſs he had lately made in boring, were of opinion, that it would be proper for the preſent to diſcontinue all operations in the well; they therefore directed the ſtage to be taken up, the mouth of the iron pipes to be carefully plugged up, the well to be covered with oak plank, and the ground over it to be paved as before; all which was accordingly done.

Boſton,
November 28, 1786.

JAMES LIMBIRD,
Surveyor to the Corporation.

XII. *Obſervations of Miſs* Herſchel's *Comet, in Auguſt and September*, 1786. *By the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Read December 21, 1786.

THE comet of Auguſt laſt, having afforded me an opportunity of putting to ſome teſt the ſyſtem of wires, a deſcription of which I laid before this Society *, I think it may not be improper, as a ſequel to that Paper, to give an account of the obſervations I have made with it on this occaſion. Though, I believe, I have nearly aſcertained with it the poſition of about 200 ſmall ſtars in the Corona Borealis; yet ſuch obſervations could not be examined by other gentlemen, without their going over the ſame ground. But this comet, which muſt have been obſerved by many, will ſerve to ſhew them, whether I have come near to the truth in this way; and what dependence may be had upon obſervations made with ſuch an inſtrument.

The teleſcope to which I applied it was an achromatic object-glaſs of DOLLOND, of 16 inches focal length, and 2 inches aperture, with a RAMSDEN's eye-glaſs, magnifying about 25 times, mounted on a very firm equatorial ſtand: with this, which takes in two degrees of a great circle, I compared the times of the comet and ſuch ſtars as lay convenient, as they

* See vol. LXXV. p. 346.

ſeverally

ſeverally paſſed the centre wire and other adjoining wires; making occaſionally a diagram, or drawing of their appearance in that teleſcope, in the manner repreſented in a Paper communicated to this Society in the year 1784*. This I found of great ſervice; both in aſcertaining the ſtars of compariſon, and in catching ſomething of an obſervation on thoſe evenings, which were not clear enough, or ſteady enough, for me to take the tranſits (as Auguſt 14. and 19. and Sept. 21.). In this way alone, one may be certain of the relative poſitions to five minutes of a degree in right-aſcenſion or declination, under the moſt unfavourable circumſtances, and commonly *much* nearer; and this occaſionally has its uſe.

This comet was firſt diſcovered Auguſt 1, 1786, at Slough, near Windſor, by Miſs CAROLINE HERSCHEL, Siſter to Dr. WILLIAM HERSCHEL, and aſſiſtant to him, and almoſt as zealous an aſtronomer as himſelf; who, in his abſence, ſwept the heavens with her teleſcope for that purpoſe. She obſerved it again Auguſt 2. and then communicated the news of it to her friends. Her account (as I recollect it, for I had not the original) was, that Auguſt 1. it was about equi-diſtant from ξ and ρ Urſæ Majoris, and N° 14, 15, and 16. Comæ Berenices, about 1° ſouth of the parallel of N° 15. Comæ. The news reached me on the 4th, which was cloudy; but on the 5th I ſaw the comet; and, having adjuſted the wires, compared its paſſage with the adjoining ſtars; of which the following are the reſults. The times noted down are all ſidereal; becauſe they were ſo obſerved, and the reduction of them is moſt ſimple. The ſtars of compariſon are moſtly thoſe of the Britiſh Catalogue: and to that the numbers refer; excepting a few, which I take to be ſtars to be found in the *Catalogue*

* See vol. LXXIV. p. 181.

complet d'Etoiles, par Bode, 1782; who, at the end of each constellation in FLAMSTEED's Catalogue, adds such stars, omitted by him, as have been observed by other astronomers, and carries on the numbers of each observer in succession. To them the name of the observer is here inserted. Those without any name or number are smaller stars, not in that or probably in any Catalogue.

Days.	At what hour fid.	Comet preceded or followed.	What star.	Differences in R. A. fid. time.	Differences in Decl.	
1786	h. ′ ″			′ ″	° ′ ″	
♄ Aug. 5	19 27 40	Com. prec.	a * –	7 2	9 32	North of the star.
			N°12, Com. Ber.	9 17	55 37	N.
			N° 16. –	13 45		
	19 51 57	Com. prec.	N° 13. –	11 4		
			N° 14. –	13 3,5	28 27	S.
			N° 16. –	13 43	2 26	S.
☉ – 6	18 30 44	Com. prec.	a * –	0 5		N. about 1° 10′.
			a * –	0 6	27 11	N.
			N°50 (Darquier)	0 26,5	2 26	S.
			N° 14. –	6 14	14 31	S.
			N° 16. –	6 49,5	12 2	N.
	19 9 23	Com. prec.	a * –	0 1		
			N°50 (Darquier)	0 22,5	1 53	S.
			N° 13. –	4 1,5		
			N° 14. –	6 10	14 13	S.
			N° 16. –	6 44,7	12 39	N.
☽ – 7	19 5 10	Com. foll.	N° 14. –	0 48	0 39	S.
			N° 16. –	0 12	26 14	N.
♂ – 8	18 21 22	Com. foll.	N° 14. –	7 29	11 2	N.
			N° 16. –	6 53	37 52	N.
	18 35 52	Com. foll.	N° 14. –	7 33	11 22	N.
			N° 16. –	6 50,5	38 15	N.
	18 51 30	Com. foll.	N° 14. –	7 38,7	10 53	N. } There is some error here, for which I cannot account.
			N° 16. –	7 3,4	37 49	N. }
♀ – 11	18 30 42	Com. foll.	N° 30. –	4 29,2	21 27	N.
			N° 31. –	2 5	22 7	N.
	18 38 15	Com. foll.	N° 30. –	4 31,7	21 25	N.
			N° 31. –	2 7,5	22 8	N.
	19 9 49	Com. foll.	N° 14. –	27 51	0 38 13	N.
			N° 16. –	27 15	1 4 0	N. Quære this Decl.

Days.	At what hour fid.	Comet preceded or followed.	What ſtar.	Differences in R.A. fid. time.	Differences in Decl.	
1786	h. ′ ″			′ ″	′ ″	
♀ Aug. 11	19 9 49	Com. foll.	N° 30. –	4 41	21 34	N.
			N° 31. –	2 16	22 16	N.
☽ – 14	19 6 0	Com. foll.	N° 43. about	0 50	22 0	N. *per* diagram only.
♄ – 19	19 10 0	Com. foll.	α ✱ Can. venat.	1 50	12 0	N.
			Nebula, N° 3.	0 20	0 20	S. All theſe were only judged from a diagram.
		Com. prec.	a ſmall ✱ ,	0 10	0 20	N.
	19 55 0	Com. foll.	α ✱ .	2 5	12 0	N.
			Nebula, N° 3.	0 35	0 20	S.
		Com. foll.	the ſmall ✱	0 5	0 20	N.
☉ – 20	18 44 2	Com. foll.	α ✱ can. venat.	7 19	9 36	N.
			Nebula, N° 3.	5 51	9 13	S. Quære this.
			α ✱ –	3 48 :		
	18 55 29	Com. foll.	α ✱ –	7 23	9 55	N.
			Nebula, N° 3.	5 51	7 42	S.
			α ✱ –	3 52	38 13	N.
♂ – 22	18 33 18	Com. foll.	N° 9. Bootis	2 20	39 6	N.
		Com. prec.	N° 11. –	2 20,5	46 26	N.
	18 45 38	Com. foll.	α ✱ –	5 42,5	31 37	S. In this ſet there is probably an error of 1′ in the Decl. of the com.
			α ✱ –	2 49	32 26	S.
			N° 9. –	2 23	38 22	N.
		Com. prec.	N° 11. –	2 17	45 48	N.
	18 58 18	Com. foll.	α ✱ –	5 45	30 34	S.
			α ✱ –	2 51		
			N° 9. –	2 26	39 26	N.
	19 8 31	Com. foll.	N° 9. –	2 29	39 31	N.
		Com. prec.	N° 11. –	2 11	47 3	N.
☿ – 23	19 6 26	Com. foll.	N° 11. –	3 6	41 32	N.
♃ – 24	19 2 22	Com. foll.	N° 9. –	13 1	28 49	N.
			N° 11. –	8 20	36 4	N.
			N° 72 (Darquier)	6 2	28 59	N.
			α ✱ –	2 28	26 32	S.
♀ – 25	19 13 24	Com. foll.	N° 9. –	18 19	23 46	N.
			N° 11. –	13 38	31 5	N.
			α ✱ –	11 20	12 45	N.
			α ✱ –	8 22,5	10 49	N.
			α ✱ –	7 46,5	21 25	S.
		Com. prec.	α ✱ –	3 57	29 32	S.
			α ✱ –	9 19 :	41 9 :	N.
			N 84 (Darquier)	9 26	32 43	N.

Days.

Days.	At what hour ſid.	Comet preceded or followed.	What ſtar.	Differences in R.A. ſid. time.	Differences in Decl.	
1786	h. ′ ″			′ ″	″	
♀ Aug. 25	19 27 45	Com. prec.	α ✱ –	3 56	30 51	N.
	19 27 43	Com. prec.	a ſmall ✱	9 13,5	39 54	N.
			N°84 (Darquier)	9 24	31 50	N.
	19 43 39	Com prec.	N°79 (Darquier)	0 40	10 53	N.
			α ✱ –	3 53	31 15	S.
♂ – 29	19 20 27	Com. foll.	α ✱ –	1 56		
		Com prec.	N°91 (Darquier)	2 13,5	0 44	S.
♃ – 30	18 1 30	Com. foll.	N°91 (Darquier)	2 30	8 5	S.
		Com. prec.	α Bootis –	6 24	19 0	N. } windy.
	18 15 49	Com. prec.	N°91 (Darquier)	2 27	7 36	S. } windy.
		Com. prec.	α Bootis –	6 14	19 9	N.
♃ – 31	19 18 24	Com. prec.	– –	1 29,5	10 49	N. } I cannot recon-
	19 21 37	Com. prec.	– –	1 30	11 6	N. } cile theſe. It
	19 24 41	Com. prec.	– –	1 30 :	9 22	N. } was very windy.
☿ Sept. 1	19 56 14	Com. foll.	– –	3 12	0 26	N. Quere the Decl.
	20 5 50	Com. foll.	– –	3 12	1 46	N.
♄ – 2	19 47 7	Com. foll.	– –	7 32	8 27	S.
		Com. prec.	✱ –	0 24		
	20 7 10	Com. foll.	– –	7 36	8 40	S.
		Com. prec.	α ✱ –	0 17	29 14	N.
♃ – 21	20 45 6	Com. foll.	τ Serpentis 1ma	1 32	58 0	N. *per* diagram only.

The comet growing faint, I did not follow it any farther. During the whole time, it was inviſible to the naked eye, and without any tail. Its appearance was ſo very ſimilar to the nebula (N° 3. in MESSIER's Catalogue inſerted in the *Connoiſſance des Temps* for 1784, and ſome other years) as ſcarcely to be diſtinguiſhed from it when in the teleſcope together; though it certainly had a brighter ſpot in the centre.

The latter obſervations were not quite ſo ſatisfactory as the preceding; the comet growing faint. Thoſe of the nebula, Auguſt 20. were alſo ſomewhat doubtful; the nebula not having the lucid point in the centre which the comet had; and therefore not being ſo eaſy to obſerve, eſpecially in paſſing the oblique wires.

Upon the whole, I think, I may with confidence recommend ſuch an inſtrument, as very convenient for ſweeping the heavens, and pretty well aſcertaining the poſition of what one diſcovers. I have here tranſcribed many obſervations which might better have been ſuppreſſed for the credit of the reſt; but from them alone it is, that a judgement can be formed of the errors to which I was liable (which yet may poſſibly have been errors of calculation), and how far ſuch an inſtrument is deſerving of any farther recommendation. A larger teleſcope and greater magnifying power, to which I can apply theſe wires, certainly would have been capable of greater accuracy: but the field being ſmaller, the ſtars of compariſon would have been the fewer, and the ſeries of obſervations leſs connected. I have at other times uſed it with a DOLLOND's eye-glaſs, with which it takes in a field of nearly three degrees; but the magnifying power is then ſo much the leſs, that, for this kind of obſervations, I prefer that of RAMSDEN, with which I can obſerve ſtars down to what I call the tenth or eleventh magnitude, and, I think, with ſome degree of preciſion.

Chiſlehurſt, Nov. 18, 1786.

XIII. *An Account of a Thunder-ſtorm in* Scotland; *with ſome Meteorological Obſervations. In a Letter from* Patrick Brydone, *Eſq. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 18, 1787.

DEAR SIR, Lennel-Houſe, near Coldſtream, Dec. 20, 1786.

I NOW ſit down to give you ſome account of the thunder-ſtorm, which I remember to have mentioned to you in converſation, and of which you wiſhed to be more particularly informed. I do not know whether you will think it worthy of the attention of the Society; but you will be pleaſed to make whatever uſe of it you think proper. It is copied, with ſome additions, from the journal which I uſually keep in the country, and which was wrote down immediately after the event.

Tueſday, the 19th of July, 1785, was a fine ſoft morning (thermometer, at ten, 68°); about eleven, clouds began to form in the ſouth-eaſt; and between twelve and one there were ſeveral flaſhes of lightning, followed by rolling claps of thunder, at a conſiderable diſtance. I was ſitting in my ſtudy at an open window, in the ſecond ſtory, obſerving the progreſs of the ſtorm; when ſome ladies, who were in the drawing-room below, alarmed by the lightning, came up to me. I was making them obſerve, by a ſtop-watch, the time which the ſound took to reach us (which was generally from 25 to 30 ſeconds), and

aſſuring them the ſtorm was at ſo great a diſtance, that there could be no ſort of danger; when we were ſuddenly alarmed by a loud report, for which we were not prepared by any preceding flaſh: it reſembled the firing of ſeveral muſkets, ſo cloſe together, that the ear could hardly ſeparate the ſounds; and was followed by no rumbling noiſe like the other claps.

The clouds immediately began to diſſipate, and there were no more appearance of either thunder or lightning. I had ordered my horſes to be got ready, and was juſt going to mount, when a ſervant came running in to tell me, that a man and two horſes had been ſtruck dead by the thunder, at a ſmall diſtance from the houſe. I immediately ſet out, and arrived on the ſpot in leſs than half an hour after the accident. The horſes were ſtill yoked to the cart, and lying in the ſame poſition in which they had been ſtruck down; but the body of the young man had been already carried off by his companion, who ſoon returned to the place; and, with leſs agitation than I expected, deſcribed to me how every thing had paſſed.

They were both ſervants to Mr. TURNBULL, a tenant of the Earl of HOME, and were returning home with two carts loaded with coals. JAMES LAUDER, a ſtrong young man, of twenty-four years of age, had the charge of the firſt cart, and was ſitting on the fore-part of it. They had croſſed the Tweed a few minutes before, at a deep ford, and had almoſt gained the higheſt part of an aſcent, about 65 or 70 feet above the bed of the river. They were converſing about the thunder, which they heard at a diſtance, and expreſſing a wiſh that it might be accompanied by a fall of rain, as the only means of ſaving the crop, after ſo long and ſo ſevere drought. At that inſtant he was ſtunned by a loud report, and ſaw his companion, his horſes and cart, fall to the ground. He immediately

ran to his aſſiſtance, but found him quite dead. His face, he ſaid, was of a livid colour, his cloaths were torn to pieces, and he had a ſtrong ſmell of burning. He immediately emptied his own cart, and carried home LAUDER's body to his friends; ſo that I had not an opportunity of examining it: but Mr. BELL, Miniſter of Coldſtream, a gentleman of the moſt perfect candour and veracity, told me, that he had been ſent for, to announce the fatal event to the young man's parents, and had examined the body; that he found the ſkin of the right thigh much burnt and ſhrivelled, and many marks of the ſame kind over the whole body; but none on the legs, which he imputed to their hanging over the fore-part of the cart at the time of the exploſion, and not being in contact with any part of it. His cloaths, and particularly his ſhirt, was very much torn, and emitted a ſtrong ſmell of burning. The body was buried two days after, without having diſcovered any ſymptoms of putrefaction.

LAUDER's companion ſhewed me the diſtance between the two carts, which was exactly marked; for his horſes had turned round at the time of the exploſion, and broke their harneſs: I found it about twenty-four yards, and LAUDER's cart was a few feet higher on the bank, but had not yet reached the ſummit. He told me, he was likewiſe ſitting on the fore-part of his cart, and had LAUDER, his cart and horſes, full in view, when they fell to the ground; that he perceived no flaſh, nor appearance of fire, and was ſenſible of no ſhock, nor uncommon ſenſation.

I now examined the cart, and the ſpot around it, as exactly as I could. The horſes were black, and of a ſtrong make; they had fallen on the left ſide, and their legs had made a deep impreſſion in the duſt, which, on our lifting them up,

ſhewed

ſhewed the exact form of each leg; ſo that no kind of ſtruggle or convulſive motion had ſucceeded the fall, but every principle of life ſeems to have been extinguiſhed in an inſtant. The hair was much ſinged over the greateſt part of their bodies; but was moſt perceptible on the belly and legs. Their eyes were already become dull and opake, and looked like the eyes of an animal which had been long dead. The joints were all ſupple; and I could not perceive that any of the bones were either ſoftened or diſſolved, as it has been alledged ſometimes happens to animals killed by lightning. The left ſhaft of the cart was broken; and I obſerved, that ſplinters had been thrown off in many places, particularly where the timber of the cart was connected by nails, or cramps of iron. Many pieces of the coal were likewiſe thrown out to a conſiderable diſtance, all round the cart; and ſome of them, which I have preſerved, have the appearance of coal which had lain ſome time on a fire. I likewiſe gathered up the fragments of LAUDER's hat, which had been torn to innumerable ſmall pieces; ſome of which I ſhall incloſe for your inſpection, as well as part of his hair, which I found ſtrongly united to ſome of the fragments which had compoſed the crown of the hat *. About four feet and a half behind each wheel of the cart, I obſerved an odd appearance in the ground; a circular hole of about twenty inches in diameter, the center of which was exactly in the tract of each wheel. The earth was torn up, as if by violent blows of a pick-axe, and the ſmall ſtones and duſt were ſcattered on each ſide of the road. The tracks of the wheels were ſtrongly marked in the duſt, both behind and before theſe holes, but were completely obliterated for upwards of a foot and a half on theſe ſpots. This led me to ſuſpect, that the force which had formed them muſt likewiſe have

* Theſe were exhibited to the Society. C. B.

acted

acted strongly upon the wheels; and, on examination, I found evident marks of fusion on each of them, which I now shewed to many people who had assembled around us. The surface of the iron, to the length of about three inches, and the whole breadth of the wheel, had become of a bluish colour, had entirely lost its polish and smoothness, and had the appearance of drops incompletely formed on its surface; these were of a roundish form, and had a sensible projection. I suspected that the great heat, which had been communicated to the iron, might probably have burnt the wood of the wheels; but this I did not find to be the case. To ascertain whether these marks were occasioned by the explosion which had torn up the ground, we pushed back the cart on the same tracks which it had described on the road; and found, that the marks of fusion answered exactly to the center of each of the holes; and that, at the instant of the explosion, the iron of the wheels had been sunk deep in the dust. They had made almost half a revolution after the explosion, which might be occasioned by the falling down of the horses, which pulled the cart a little forward. On examining the opposite part of the wheels, or that part which was at the greatest distance from the earth, no mark of any kind was observable. The broken earth still emitted a smell something like that of ether. The ground was remarkably dry, and of a gravelly soil.

It would appear, that this great explosion had, in an instant, pervaded every substance connected with the cart, the wheels of which had probably conducted it from the ground. They had been completely wetted but a few minutes before, as well as the legs and bellies of the horses, and this might, perhaps, be the reason why the hair on these parts was much more burnt than on the rest of their bodies. However, the two horses

had already walked over this electrical mine, without having produced any effect; and had not the cart followed them might have escaped without hurt. I examined all their shoes, but could not perceive the least mark on any of them, nor was the earth broken where they had trodden. But the cart was deeply laden, and the wheels had penetrated much farther into the ground.

The equilibrium between the earth and the atmosphere seems at this instant to have been completely restored; for no farther appearance of thunder or lightning was observed within our hemisphere; the clouds dispelled, and the air resumed the most perfect tranquillity: but how this vast quantity of electric matter could be discharged from the one element into the other without exhibiting any appearance of fire, I shall not pretend to examine. The fact, however, appears certain; and when I was mentioning it as a singular one, a gentleman told me, that the shepherd of St. Cuthbert's farm, on the opposite bank of the Tweed, had been an eye-witness of the event, and gave a different account of it. I immediately went to the farm, found the shepherd, and made him conduct me to the spot from whence he had observed it, and desired him to give me an account of what had happened. He was looking, he said, at the two carts going up the bank, when he was stunned by a loud report, and at the same instant saw the first of the carts fall to the ground, and observed that the man and horses lay still, as if dead. I asked him, if he had observed any lightning? He said, he saw no lightning, nor appearance of fire whatever; but observed the dust to rise at the place; that there had been several flashes of lightning some time before from the south-east, whereas the accident happened to the north-west of where he stood. The distance, in a right line

acroſs the river, might be between two and three hundred yards. He was ſenſible of no ſhock, nor uncommon ſenſation of any kind. I went next morning to examine if there were any marks of putrefaction on the horſes, and to obſerve the ſtate of the blood-veſſels, &c. after the ſkin had been taken off; but a gentleman of the neighbourhood, who kept a pack of hounds, had already ſeized on them.

Several other phænomena happened on that day, probably all proceeding from the ſame cauſe; ſome of which I ſhall beg leave to mention.

The ſhepherd, belonging to the farm of Lennel-Hill, was in a neighbouring field, tending his flock, when he obſerved a lamb drop down; and ſaid, he felt at the ſame time as if fire had paſſed over his face (this was his own expreſſion), although the lightning and claps of thunder were then at a great diſtance from him. He ran up immediately, but found the lamb quite dead; nor did he perceive the leaſt convulſive motion, nor ſymptom of life remaining, although the moment before it appeared to be in perfect health. He bled it with his knife, and the blood flowed freely. This, he told me, happened about a quarter of an hour before the exploſion which killed LAUDER; and it was not above three hundred yards diſtant from the ſpot. He was only a few yards from the lamb when it fell down. The earth was not torn up, nor did he obſerve any duſt riſe.

THOMAS FOSTER, a celebrated fiſher in Coldſtream, and another man, were ſtanding in the middle of the Tweed, fiſhing for ſalmon with the rod, when they ſuddenly heard a loud noiſe; and, turning round to ſee from whence it came, they found themſelves caught in a violent whirlwind, which FOSTER told me felt ſultry and hot, and almoſt prevented

them from breathing. It was not without much difficulty they could reach the bank, where they sat down, exhausted with fatigue, and greatly alarmed: however it lasted but a very short time, and was succeeded by a perfect calm. This happened about an hour before the explosion.

A woman, making hay near the banks of the river, fell suddenly to the ground, and called out to her companions, that she had received a violent blow on the foot, and could not imagine from whence it came. This I had not from the woman herself, but from Mr. TURNBULL, a very respectable farmer. Mr. BELL, our minister, nephew of THOMSON the Poet, and possessed of all the candour and ingenuity of his uncle, told me, that, walking in his garden, a little before LAUDER's accident, he several times felt a sensible tremor in the ground. He likewise told me (what I find I had forgot to mention in the proper place), that he had observed on LAUDER's body a zig-zag line, of about an inch and a quarter broad, which extended from his chin down to his right thigh, and had followed nearly the line of the buttons of his waistcoat. The skin was burnt white and hard.

These, sir, are all the circumstances I have been able to collect that are well authenticated; and I shall not trouble you with reports that are not. From the whole it would appear, that the earth had acquired a great super-abundance of electrical matter, which was every where endeavouring to fly off into the atmosphere. Perhaps it might be accounted for from the excessive dryness of the ground; and, for many months, the almost total want of rain, which is probably the agent that Nature employs in preserving, or in restoring, the equilibrium between the other two elements. But I shall not pretend to investigate the causes: all I wanted, was to give you some account

account of the effects; and your own reflections will lead you much farther than any thing I could ſuggeſt.

I have the honour to be, with the greateſt reſpect, &c.

P. BRYDONE.

P.S. I cannot ſend away this letter without adding, in a Poſtſcript, that on Friday the 11th of Auguſt laſt, early in the morning, we had a pretty ſmart ſhock of an earthquake. I was awaked by it, and felt the motion moſt diſtinctly for four or five ſeconds at leaſt. It appeared as if the bed had been pulled gently from ſide to ſide ſeveral times. The motion was nearly north north-weſt and ſouth-eaſt, as far as I could judge from the motion of the bed. The windows were violently ſhaken, and made a great noiſe, which, I believe, was miſtaken by many people for a noiſe accompanying the earthquake. I immediately roſe to look at my watch, and found it twenty minutes after two. It was a dead calm, the morning cloſe and warm, with ſmall drizzling rain, and, although the moon was but two days paſt the full, ſo dark that I could not perceive the hour without ſtriking a light. It was felt in almoſt every houſe in this neighbourhood, and all the way from this country to the weſt coaſt of the iſland, where it ſeems to have been more violent than here; but to the eaſt of this place it was very little felt.

Perhaps it may not be improper to mention the ſtate of the weather for ſome time before and after this event, as it may poſſibly have had ſome influence upon it. The drought was very great till the 22d of July, when it rained a little; and this was repeated, though in ſmall quantities, and generally accompanied by high winds, till Thurſday the 27th, when it blew

blew the moſt violent tempeſt I ever remember in this country. The young crop of turneps, in many fields, were blown out of the ground, and almoſt entirely deſtroyed. The peaſe became brown as if withered, and ſo did the leaves of the foreſt trees on that ſide which was oppoſed to the blaſt. Vaſt clouds of duſt were raiſed from the dry fields and roads, which looked like ſmoke, and had the appearance at a diſtance as if many villages had been on fire all over the country. The water too was raiſed from the ſurface of the river, and carried quite away by the violence of the hurricane, forming ſmall clouds in the air, which we traced to a great diſtance. The great violence of this tempeſt laſted but a few hours, and at night it fell calm. The barometer was little affected, and ſtood at 29 inches and a half. The wind was nearly weſt, veering ſometimes a little to the north. From this time we had a tract of very fine weather, the wind conſtantly in the weſt points, till the time of the earthquake (which happened on what is called the laſt of the dog days), when it changed to the ſouth-eaſt, and brought us five of the worſt days I ever remember to have ſeen at that ſeaſon; it rained almoſt inceſſantly, with a cold eaſterly wind, and the ſun did not once appear till the morning of Wedneſday the 16th, after which we had again a tract of fine weather. I examined the barometer at the time of the earthquake, but did not find that it had been ſenſibly affected. It roſe a little on that morning; but this I imputed to the wind having changed into the eaſt.

XIV. *On finding the Values of Algebraical Quantities by converging Series, and demonstrating and extending Propositions given by* Pappus *and others.* By Edward Waring, *F.R.S. Professor of Mathematics at* Cambridge.

Read February 8, 1787.

SUPPOSE the roots of the equation $x^b \pm 1 = 0$ to be given, where b denotes any whole number or fraction; to find the roots or values of any given algebraical quantity, by converging infinite series.

1. Let the algebraical quantity be $\sqrt[a]{(\pm A)}$, then the roots of the algebraical quantity will be $A^{\frac{1}{a}} \times (\alpha + \lambda\sqrt{-1})$, $A^{\frac{1}{a}} \times (\beta + \mu\sqrt{-1})$, $A^{\frac{1}{a}} \times (\gamma + \nu\sqrt{-1})$, &c. where $\alpha + \lambda\sqrt{-1}$, $\beta + \mu\sqrt{-1}$, $\gamma + \nu\sqrt{-1}$, &c. are the roots of the equation $x^a \pm 1 = 0$; it will be $+1$ if it was $-A$, and -1 if $+A$.

2. Let the given algebraical quantity be $\sqrt[a]{(\pm\sqrt[a]{(\pm A)} \pm \sqrt[a]{(\pm B)} \pm \sqrt[a]{\pm C} \pm$ &c.), and $\alpha + \lambda\sqrt{-1}$, $\alpha' + \lambda'\sqrt{-1}$, $\alpha'' + \lambda''\sqrt{-1}$, &c. and $P + \Delta\sqrt{-1}$ be respectively one of the roots of the equations $x^a \mp 1 = 0$, $x^a \mp 1 = 0$, $x^a \mp 1 = 0$, &c. and $x^a \mp 1 = 0$; substitute $\pm P = \pm A^{\frac{1}{a}}\alpha \pm B^{\frac{1}{a}}\alpha' \pm C^{\frac{1}{a}}\alpha'' \pm$ &c. and $\pm Q = \pm A^{\frac{1}{a}}\lambda \pm B^{\frac{1}{a}}\lambda' \pm C^{\frac{1}{a}}\lambda'' \pm$ &c. In the first place let P be greater Q, and $\pm P$ be $+P$, then will $(P \pm Q\sqrt{(-1)})^{\frac{1}{a}} =$

(P

$$\left(P^{\frac{1}{r}} - \frac{1}{r}\cdot\frac{1-r}{2r}\times\frac{Q^2}{P^{\frac{2r-1}{r}}} + \frac{1}{r}\cdot\frac{1-r}{2r}\cdot\frac{1-2r}{3r}\cdot\frac{1-3r}{4r}\times\frac{Q^4}{P^{\frac{4r-1}{r}}} - \&c.\right.$$

$$\left. = \pm L\right) \pm \left(\frac{1}{r}\cdot\frac{Q}{P^{\frac{r-1}{r}}} - \frac{1}{r}\cdot\frac{1-r}{2r}\cdot\frac{1-2r}{3r}\cdot\frac{Q^3}{P^{\frac{3r-1}{r}}} + \frac{1}{r}\cdot\frac{1-r}{2r}\cdot\right.$$

$$\left.\frac{1-2r}{3r}\cdot\frac{1-3r}{4r}\cdot\frac{1-4r}{5r}\times\frac{Q^5}{P^{\frac{5r-1}{r}}} - \&c. = \pm M\right)\times\sqrt{-1} = \pm L \pm M\sqrt{-1},$$

in which caſe the two ſerieſes $\pm L$ and $\pm M$ converge, and $(\Gamma + \Delta\sqrt{-1}) \times (\pm L \pm M\sqrt{-1})$ will be a value or root of the given quantity.

In the ſame manner the remaining roots may be deduced.

2. Let $\pm P$ be $-P$, multiply $-P \pm Q\sqrt{(-1)}$ into -1, and it becomes $P \mp Q\sqrt{(-1)}$ a quantity of the ſame formula as the preceding; let $\Gamma' + \Delta'\sqrt{(-1)}$ be a root of the equation $x^r + 1 = 0$, then will $(\Gamma' + \Delta'\sqrt{-1})(\pm L \pm M\sqrt{-1}) = \pm H' \pm K'\sqrt{(-1)}$ be a root of the given quantity: otherwiſe; the root may be deduced from the above-mentioned ſeries by ſubſtituting in it for $-(P)^{\frac{1}{r}}$ its value $P^{\frac{1}{r}} \times (-1)^{\frac{1}{r}}$, and it will become the ſame as the preceding.

3. Let P be leſs than Q, and the value of $(\pm P \pm Q\sqrt{-1})^{\frac{1}{r}}$ may be deduced from the preceding ſeries by ſubſtituting in it $\pm Q\sqrt{-1}$ for P, and $\pm P$ for Q: otherwiſe, ſince $(\pm P \pm Q\sqrt{(-1)})^{\frac{1}{r}} = \pm\sqrt[2r]{(-1)} \times (Q \mp P\sqrt{(-1)})^{\frac{1}{r}}$, and the root of $(Q \mp P\sqrt{(-1)})^{\frac{1}{r}}$ can be deduced by the preceding method, which ſuppoſe $L' + M'\sqrt{(-1)}$; multiply this root into $H \pm \Theta\sqrt{(-1)}$, where $H + \Theta\sqrt{(-1)}$ denotes a value of the root $\pm\sqrt[2r]{(-1)}$, and the quantity reſulting will be

be one value of the given quantity; the remaining values can be deduced by the same method.

In this case the given quantity is resolved into a series ascending according to the dimensions of P, and descending according to the dimensions of Q; in the former case it was resolved into a series ascending according to the dimensions of Q, and descending according to the dimensions of P; both the series affording the possible or impossible parts will always converge.

4. If $P = \pm Q$, then will $(\pm P \pm P\sqrt{(-1)})^{\frac{1}{r}} = P^{\frac{1}{r}} \times (\pm 1 \pm \sqrt{(-1)})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} (\pm\sqrt{1} \pm \sqrt{-1})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} \times \sqrt[4r]{(-1)}$; for $\sqrt[4]{(-1)} = \pm\sqrt{1} \pm \sqrt{-1}$.

4. 2. When $P = 0$, or $Q = 0$, then it becomes the first case $\sqrt[r]{(\pm A)}$.

5. Let $P = Q \mp \alpha$, where α has a very small ratio to Q; then will $(P \pm Q\sqrt{(-1)})^{\frac{1}{r}} = (P \pm \overline{P \pm \alpha}\sqrt{(-1)})^{\frac{1}{r}} = (P \times 2^{\frac{1}{2}} \times \overline{-1}^{\frac{1}{2}} \pm \alpha\sqrt{(-1)})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} \times \sqrt[4r]{(-1)} \pm \frac{1}{r} \times P^{\frac{1-r}{r}} \times 2^{\frac{1-r}{2r}} \times \sqrt[4r]{(-1)} \times \sqrt{(-1)}\,\alpha - \frac{1}{r} \times \frac{1-r}{2r} \times P^{\frac{1-2r}{r}} \times 2^{\frac{1-2r}{2r}} \times \sqrt[4r]{(-1)} \times \alpha^2 \pm \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \frac{1-2r}{3r} \times P^{\frac{1-3r}{r}} \times 2^{\frac{1-3r}{2r}} \times \sqrt[4r]{(-1)} \times \sqrt{(-1)}\,\alpha^3 + \&c.$ In this series the same root of the quantity $\sqrt[4r]{(-1)}$ is always to be used.

6. If in the given quantity are contained more quantities of the above-mentioned kind or their roots; then, by repeating the same operation, can be deduced the roots or values of the given quantity.

In ſome caſes the impoſſible part may vaniſh, which may be the caſe in a quantity of the following formula, *viz.* $\sqrt[n]{a+\alpha\sqrt[2m]{-b}}+\sqrt[n]{a+\beta\sqrt[2m]{-b}}+\sqrt[n]{a+\gamma\sqrt[2m]{-b}}+$ &c. where α, β, γ, &c. denote the $2m$ roots of $\sqrt[2m]{-1}$. The general principles of diſcovering the caſes in which this happens have been given in the Meditationes Algebraicæ.

The roots of the equation $x^b \pm 1 = 0$ will be found from common algebra and theſe principles, if b is not greater than 10; or, more generally, if $b = 2^l \times 3^{l'} \times 4^{l''} \ldots 10^{l^8}$, where l, l', l'' .. l^8 denote any whole numbers: or, in general, the roots of the above-mentioned equation, or even of the equation $x = \sqrt{\pm L \pm M\sqrt{-1}}$, can be found from tables of ſines.

The ſame principles may be applied to the diſcovery of the values of exponential irrational quantities.

In the Miſcel. Analy. was given, from a ſubſtitution invented by me and not ſimilar to any before given, a reſolution of equations, which contains the reſolutions of all equations before given, and from which the reſolutions of ſome equations, not before delivered, have been added.

Part II. 1. Let an equation $A = 0$ involving (r) unknown independent quantities be predicated of another equation containing the ſame quantities, and the demonſtration of it be required.

1ſt. Reduce both the equations to equations involving independent quantities only; then reduce the two equations to one, ſo that one of the above-mentioned quantities may be exterminated, and if there reſults a ſelf-evident equation, *viz.* $A = A$, or $A - A = 0$, in which the correſpondent terms deſtroy each other reſpectively; then the firſt equation is juſtly predicated of the ſecond; that is, if the above-mentioned

equations

equations afford the same value of the quantity exterminated, the proposition is true, otherwise not.

Cor. From these principles can be demonstrated many propositions given by PAPPUS and others.

Ex. Let AD=2AC=2x, DE=a, and EB=b, where AD, DE, and EB, are independent quantities; if AB × BE=$(2x+a+b) \times b = \text{CB} \times \text{BD} = (x+a+b)(a+b)$, then will CB=$x+a+b$: BD=$a+b$ :: AC × CE=$x \times (x+a)$: AD × DE=$2x \times a$. From hence can be deduced the two equations $(b-a)x=a^2+ab$ and $2a \times (x+a+b)=(a+b) \times (x+a)$; reduce these two equations to one, so as to exterminate x, and there results the self-evident equation $(a-b) \times \frac{a^2+ab}{b-a}(-a^2-ab) + a^2+ab=0$, and consequently the proposition is true.

A C D E B

2. If (s) equations involving ($t+r$) unknown and independent quantities be predicated of (t) equations involving the above-mentioned quantities: reduce the (t) equations and one of the above-mentioned (s) equations to one, so that (t) unknown quantities may be exterminated, and if there results a self-evident equation, then the above-mentioned equation is justly predicated of the (t) equations; and in the same manner we may reason concerning the remaining ($s-1$) equations.

3. 1. If one equation is justly predicated of another, and in both the unknown quantity exterminated has only one dimension; then the latter equation can be predicated of the former; for in this case both equations have only one and the same value of the unknown quantity exterminated.

3. 2. If the quantity exterminated has more dimensions than one in the equations, then the proposition may not gene-

rally be true; for the equations may have ſome roots the ſame, but not all.

Theſe obſervations may be applied to more equations.

4. From (n) given equations $a=0$, $b=0$, $c=0$, &c. can eaſily be deduced others dependent on them, by finding any direct algebraical functions of the above-mentioned equations, that is, ϕ (a, b, c, &c.), which will always $=0$; and in like manner, from the relation between any lines being given, can be deduced innumerable relations between the above-mentioned lines, and other lines dependent on them.

Part III. 1. Ratios, which are ſuppoſed greater or leſs than others, can eaſily be transformed into equations, which contain affirmative and negative quantities: for example, let the ratio $a : b$ be greater than the ratio $c : d$, then will $\frac{b}{a} = \frac{c}{d} - k$; if it be leſs, then will $\frac{b}{a} = \frac{c}{d} + k$, where k denotes an affirmative quantity; and, *vice verſâ*, if $\frac{b}{a} = \frac{c}{d} - k$, then will the ratio of $a : b$ be greater than the ratio of $c : d$, &c.

2. If one quantity (a) is affirmed to be greater than another b, for a in the given equations ſubſtitute its value $b+k$; if leſs, for a write $b-k$, where k denotes an affirmative quantity.

3. Reduce the equations, ſo as to take away their denominators, and the demonſtration of the propoſition will often very eaſily follow.

4. Let $k = \frac{P}{Q}$ and $k' = \frac{P'}{Q'}$; and if P and Q be affirmative, let P′ and Q′ be affirmative; and, *vice verſâ*, if negative, negative; then, if k be affirmative, will k' alſo be affirmative; the ſame alſo may be affirmed, if P and Q have both contrary

trary signs to P′ and Q′; but if one has the same, and the other contrary, then will k and k' have contrary signs.

5. Let some affirmative quantities be less than others, then any direct affirmative function of the former, *viz.* function, in which no negative or impossible quantities or indexes are contained, will be less than the same function of the latter. The contrary happens when the indexes are all negative, and the quantities affirmative as before: for example, let two quantities be less than two others, then the product of the two former will be less than the product of the two latter.

Cor. Hence some quantities may often be known to be greater or less than others from their direct functions being greater or less than the same functions of the others: for example, let $a^2 - b^2$ be an affirmative quantity, then will a be greater than b.

6. If one equation or ratio is affirmed on the supposition that another given one is true, reduce both the equations by the methods given above, and from the principles before delivered, the proposition will often be evident.

Hence may be deduced demonstrations to propositions of this sort given by PAPPUS and others.

Ex. Let the ratio $a+b : b$ be greater than $c+d : d$, then the ratio $b : a-b$ will be less than $d : c-d$.

For, since the ratio $a+b : b$ is greater than $c+d : d$, the ratio $b : a+b$ will be less than $d : c+d$, and consequently $\frac{a+b}{b}\left(\frac{a}{b}+1\right) = \frac{c+d}{d}\left(\frac{c}{d}+1\right)+k$, whence $\frac{a}{b}-1 = \frac{c}{d}-1+k$, and $\frac{a-b}{b} = \frac{c-d}{d}+k$, and the ratio $b : a-b$ less than $d : c-d$.

Ex. 2. Let the ratio of $a+b : c+d$ be greater than the ratio of $a : c$, then will the ratio of $b : d$ be greater than the ratio of $a+b : c+d$. By the preceding method convert these ra-

tios into equations, and there reſult $\frac{c+d}{a+b}+k=\frac{c}{a}$ and $\frac{d}{b}+k'=\frac{c+d}{a+b}$; and the propoſition aſſerts, that if k be an affirmative quantity, k' will alſo be an affirmative quantity. Reduce theſe two equations, ſo as to take away their denominators, and the reſulting equations will be $ac+ad+a\times\overline{a+b}\times k=ac+bc$ and $ad+bd+\overline{a+b}\,.\,bk'=bc+bd$, whence $k=\frac{bc-ad}{a\,.\,a+b)}$ and $k'=\frac{bc-ad}{b\,(a+b)}$, and the propoſition is evident.

Ex. 3. Let a be greater than c, and b, and $(a+b)\times(a-b)=(c+d)\times(c-d)$, that is, $a^2-b^2=c^2-d^2$, then will b be greater than d; for a in the equation $a^2-b^2=c^2-d^2$ write $c+k$, and there reſults $2ck+k^2=b^2-d^2$, whence b^2-d^2 is an affirmative quantity, and conſequently b greater than d.

Ex. 4. Let, as in Ex. 1. the ratio $a+b:b$ be greater than $c+d:d$, then will $b:a-b$ be leſs than the ratio $d:c-d$. By the preceding method tranſlate theſe ratios into the two equations $\frac{b}{a+b}+k=\frac{d}{c+d}$ and $\frac{a-b}{b}=\frac{c-d}{d}+k'$, reduce theſe equations, ſo as to take away their denominators, and there reſult $bc+bd+\overline{a+b}\times\overline{c+d}k=ad+bd$ and $da-db=bc-bd+bdk'$, and conſequently $k=\frac{ad-bc}{(a+b)\,(c+d)}$ and $k'=\frac{ad-bc}{bd}$; but theſe two fractions which expreſs the values of k and k' have the ſame numerators, and their denominators both affirmative; therefore, if one k be affirmative, the other k' will alſo be affirmative.

Cor. From theſe principles can eaſily be deduced innumerable propoſitions of this ſort. Aſſume two or more ratios, of which let ſome be ſuppoſed greater than others; then, from the above-mentioned transformation, by addition, ſubtraction, multiplication, diviſion, &c. can be found ſuch functions of the

the above-mentioned quantities, that some may become greater than others, and thence may be deduced the propositions above-mentioned.

7. It may not be improper in this place to adjoin a few observations on finding the limits of some quantities in which others contained in given equations become negative or affirmative.

1. Given an equation involving two unknown quantities x and y; the limits of the quantity y, between which the quantity x will become affirmative or negative, may be deduced from the following principles.

The quantity x passes from affirmative to negative or from negative to affirmative, either through nothing or infinite; or from two impossible roots it passes to affirmative or negative through two or more equal roots; and, *vice versâ*, from affirmative or negative to two or more impossible roots through two or more equal roots.

Find therefore the values of y, when x becomes $= 0$, or infinite; and also all the cases in which two, &c. values of x become equal, that is, when its roots become impossible; and from thence can be deduced the limits of the quantity y, between which (x) becomes affirmative or negative.

2. If $x = \frac{P}{Q}$ be an affirmative quantity, then P will be affirmative or negative, according as Q is an affirmative or negative quantity, &c. Assume therefore $P = 0$ and $Q = 0$, and from the roots of the resulting equation can be deduced the cases, in which (x) becomes an affirmative quantity.

3. If more (n) unknown quantities (x, y, z, v, &c.) be contained in a given equation; then, by the preceding method, find the limits of (z, v, &c.), between which (x) becomes an affirmative or negative quantity, and let the quantities denoting the limits contain not more than $(n-1)$ unknown quantities:

from

from the above-mentioned quantities or equations expreſſing the limits, find others denoting their limits, which do not contain more than $(n-2)$ above-mentioned quantities, and ſo on.

4. Often from the ſubſtitution of the limits of given quantities can be acquired the limits of the remaining one (x). Find all the greateſt values of the quantity (x) contained between the above-mentioned limits, and thence can be deduced the limits ſought.

5. If there are given (m) equations involving $(m+1)$ or more unknown quantities; then ſometimes with, and ſometimes without, reducing them to others involving more few unknown quantities can be found by the preceding method limits; and from comparing the limits ſo acquired can ſometimes be deduced the limits ſought.

6. If a given function of the unknown quantities (x, y, z, &c.) is aſſerted to be contained between given limits, when other functions of the above-mentioned quantities are contained between given limits, and the demonſtration of it is required; from the given equations and the given functions find limits of the unknown quantities reſpectively, and if the latter limits are contained between the former, the propoſition is generally true, otherwiſe not.

7. From the above-mentioned principles can be found the caſes in which an unknown quantity (x) admits of one or more affirmative values.

8. It appears from the principles before delivered, that the finding the number of affirmative and negative roots of a given equation neceſſarily includes the finding the number of its impoſſible roots; and therefore it may not be improper to ſubjoin ſomewhat on what has been done on this ſubject.

1. DESCARTES

1. DESCARTES gave a method of finding the number of affirmative and negative roots of a given equation, when all its roots are poſſible; but all the roots are very ſeldom in equations of ſuperior dimenſions poſſible, unleſs when the equation is purpoſedly made.

2. It has been demonſtrated by others and myſelf, that the equation will at leaſt have ſo many changes of ſigns from + to −, and − to +, as there are affirmative roots, and ſo many continued progreſſes from + to + and − to −, as there are negative roots.

3. A rule for finding in general the number of affirmative or negative roots in a biquadratic, and in the equation $x^n + Ax^m + B = 0$, was firſt publiſhed in the Medit. Algebr.

4. HARRIOT demonſtrated a method of finding the number of impoſſible roots contained in a cubic equation. In the year 1757 I ſent to the Royal Society a method of finding the number of impoſſible roots contained in a biquadratic and quadrato-cubic equations, and in the equation $x^n \pm Ax^m \pm B = 0$.

5. SCHOOTEN gave a method of finding the number of impoſſible roots which can be concluded from the deficient terms of an equation. NEWTON gave a rule which often diſcovers the number of impoſſible roots contained in a given equation. CAMPBELL diſcovered a new rule on the ſame ſubject. Mr. MACLAURIN has added ſomewhat more general on theſe ſubjects: theſe rules may be rendered more general by a principle firſt given in the Miſcell. Analyt. *viz.* multiplying the given equation into a quantity $x - a$ or $(x - a) \times (x - b)$, &c. and finding from the rule the number of impoſſible roots contained in the given equation. Similar and more general rules and principles have been added in the Medit. Algebr. Theſe rules, in equations of ſuperior dimenſions, ſeldom diſcover the true number of im-

poſſible roots. I believe alſo, that I firſt gave a rule in the Miſcell. Analyt. for finding the number of impoſſible roots from finding an equation, whoſe roots are the ſquares &c. of the roots of a given equation, which rule in equations of ſuperior dimenſions ſometimes finds impoſſible roots, when NEWTON's, CAMPBELL's, &c. rules fail, and fails when they find them; and alſo a rule for finding impoſſible roots from an equation, whoſe roots are the ſquares of the differences of the roots of the given equation; this rule (as has been obſerved by me in the Miſcell. Analyt. and Philoſophical Tranſactions) always diſcovers whether all the roots of the given equation are poſſible or not; and the laſt term of the reſulting equation diſcovers alſo, whether 0, 4, 8, 12, &c. or 2, 6, 10, 14, &c. impoſſible roots, are contained in the given equation; to which may be ſubjoined, if the given equation has r poſſible and $n-r=2t$ impoſſible roots, that the number of changes of ſigns from + to − and − to + in the reſulting equation will not be leſs than $r \cdot \frac{r-1}{2}$, and the number of continued progreſſes from + to + and − to − will not be leſs than t: whence, if the number of continued progreſſes be t', the number of impoſſible roots will not be greater than $2t'$, and the number of poſſible roots not leſs than $n-2t'$. If the number of changes of ſigns be b', the number of poſſible roots will not be greater than r', where $r' \times \frac{r'-1}{2}$ is the greateſt poſſible number which does not exceed b', and the number of impoſſible roots not leſs than $n-r'$. Another rule was, I believe, firſt given by me in the Miſcell. Analyt. 1762, for finding impoſſible roots by finding an equation whoſe roots are z, where $x^n - px^{n-1} + qx^{n-2} -$ &c. $= z$, and $nx^{n-1} - \overline{n-1}\,px^{n-2} + \overline{n-2}\,qx^{n-3} -$ &c. $= 0$.

In

In the Medit. Algebr. ſomewhat has been added concerning impoſſible, affirmative, and negative values of the unknown quantities in an equation which involves two or more unknown quantities; and alſo was firſt delivered a rule from the number of affirmative, negative, and impoſſible roots of an equation being known to find the number of impoſſible, negative, and affirmative roots of an equation, whoſe roots have a given algebraical relation to the roots of a given equation; on which two laſt ſubjects little, I believe, had been before publiſhed.

XV. *Experiments on the Production of Dephlogiſticated Air from Water with various Subſtances. In a Letter from Sir* Benjamin Thompſon, *Knt. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read Feb. 15, 1787.

DEAR SIR, Munich, Sept. 1, 1786.

VARIOUS opinions having been entertained with reſpect to the origin of the dephlogiſticated air, produced by expoſing healthy vegetables in water to the action of the ſun's rays, according to the method of Dr. INGEN-HOUSZ; and not being myſelf thoroughly ſatisfied with any of the theories propoſed, I made the following experiments, with a view to throwing ſome new light upon that ſubject.

Having found that raw ſilk poſſeſſes a power of attracting and ſeparating air from water in great abundance, when expoſed in it to the action of light, it occurred to me to examine the properties of this air, and to conſider more attentively the circumſtances attending its production, thinking that this might poſſibly lead to ſome further diſcoveries, relative to the production of the air yielded by water under other circumſtances: and though my ſucceſs in theſe inquiries has not been equal to my wiſhes, yet, as in the courſe of my reſearches I have diſcovered ſome facts which I take to be new, and as I have confirmed others, already known, by a variety of new experiments,

ments, I flatter myſelf that you will not think an account of my labours upon this ſubject altogether unintereſting.

Before I enter upon the detail of my experiments it will be neceſſary to premiſe, that I ſhall in general confine myſelf merely to the facts as they appear, without applying them to the confirmation or refutation of the theories of others, and without entering into any ſpeculative enquiries relative to their remote cauſes; and in deſcribing the different appearances I ſhall make uſe of the moſt familiar terms. Thus, in ſpeaking of the air produced upon expoſing raw ſilk in water to the action of light, I ſhall ſometimes mention it as being yielded by the ſilk; and I ſhall ſometimes ſpeak of the air furniſhed by expoſing water, which has previouſly turned green, in the ſun's rays, as being immediately produced by the water, though it is probable, that the *green matter* acts a very important part in the production of this air in the one caſe and in the other. But how it acts is not well aſcertained; and I had in general much rather confine myſelf to a ſimple, and even an unlearned, deſcription of facts, than by endeavouring to give more preciſe definitions, at firſt, to involve myſelf in all the difficulties which would attend accounting for phænomena, whoſe cauſes are but very imperfectly known.

You will, therefore, not be ſurpriſed, if you ſhould ſometimes find me ſpeaking of appearances in the ſame manner as a perſon would mention them who ſaw them for the firſt time, and who did not know that others had diſcovered them before, and how they had endeavoured to account for them. I ſhall take care that the facts ſhall be faithfully deſcribed, and I flatter myſelf you will not think them the leſs intereſting on account of their being unadorned.—But I haſten to give you an account of my experiments.

Expe-

Experiment N° 1.

My firſt object was to collect a ſufficient quantity of the air ſeparated from water by ſilk to determine its goodneſs by the teſt of nitrous air; and to this end, having filled with clear ſpring water a globe of thin, white, and very tranſparent glaſs, 4½ inches in diameter, with a cylindrical neck ¼ of an inch in diameter, and about 12 inches long, I introduced into it 30 grains of raw ſilk, which had been previouſly waſhed in water, in order to free it of air; and inverting the globe under water, and placing its neck in a glaſs jar, containing a quantity of the ſame water with which the globe was filled, I expoſed it in my window to the action of the ſun's rays, and prepared myſelf to examine the progreſs of the generation or production of the air.

The globe had not been expoſed ten minutes to the action of the ſun's rays, when I diſcovered an infinite number of exceedingly ſmall air-bubbles, which began to make their appearance upon the ſurface of the ſilk; and theſe bubbles continuing to increaſe in number, and in ſize, at the end of about two hours the ſilk, appearing to be intirely covered with them, roſe to the upper part of the globe.

Theſe bubbles going on to increaſe in ſize, and running into each other, at length began to detach themſelves from the ſilk, and to form a collection of air at the upper part of the globe; but the meaſure of my eudiometer being rather large, it was not till after the globe had been expoſed in the ſun near four days, that a ſufficient quantity of air was collected to make the experiment with nitrous air, in order to aſcertain its goodneſs by that teſt.

Having at length collected a ſufficient quantity of this air for that purpoſe, I carefully removed it from the globe, and mixing with 1 meaſure of it 3 meaſures of nitrous air, they were reduced to 1,24 meaſures; which ſhews, that it was actually *dephlogiſticated air*, and that of a conſiderable degree of purity.

Common air, tried at the ſame time, 1 meaſure of it with 1 meaſure of nitrous air were reduced to 1,08 meaſure.

Having again expoſed the globe with the ſame water and ſilk in my window, where the ſun ſhone the greateſt part of the day, at the end of three days I had collected 3½ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,18$; that is to ſay, 1 meaſure of this air, *added to* 3 meaſures of nitrous air, were reduced to 1,18 meaſure.

A ſmall wax taper, which had been juſt blown out, a ſmall part only of the wick remaining *red-hot*, upon being plunged into a phial filled with this air, immediately took fire, and burnt with a very bright and enlarged flame.

The water in the globe appeared to have loſt ſomething of its tranſparency, and had changed its colour to a very faint greeniſh caſt, having at the ſame time acquired the odour or fragrance proper to raw ſilk.

This experiment I repeated ſeveral times with freſh water (retaining the ſame ſilk) and always with nearly the ſame reſult; with this difference, however, that when the ſun ſhone very bright, the quantity of air produced was not only greater, but its quality likewiſe was much ſuperior to that yielded when the ſun's rays were more feeble, or when they were frequently intercepted by flying clouds. The air, however, was always not only much better than common air, but better than the air in general produced by the freſh leaves of plants expoſed in water to the ſun's rays in the experiments of Dr. INGEN-

HOUSZ;

HOUSZ; and under the circumſtances the moſt favourable, it was ſo good that 1 meaſure of it required 4 meaſures of nitrous air to ſaturate it, and 3,65 meaſures of the two airs were deſtroyed; or, proved with nitrous air it gave $1a + 4n = 1{,}35$, which, I believe, is better than any air that has yet been produced in the experiments with vegetables.

The method I have here adopted of uſing algebraic characters in noting the reſult of the experiments made to determine the goodneſs of air, though not ſtrictly mathematical, is very convenient; and for that reaſon, I ſhall continue to make uſe of it. a repreſents the air which is proved; n nitrous air; and the numbers which are joined to theſe letters ſhew the quantities, or the number of meaſures, of the different airs made uſe of in the experiment. The other number, which ſtands alone, or without any letter attached to it, on the other ſide of the equation, ſhows the volume, or the number of meaſures and parts of a meaſure to which the two airs are reduced after they are mixed. I ſhall ſometimes add a fourth number, ſhewing the quantity of the two airs deſtroyed, as this more immediately ſhews the goodneſs of the air which is proved.

Thus, in the experiment laſt mentioned, 1 meaſure of the air proved, mixed with 4 meaſures of nitrous air, were reduced to 1,35 meaſure, conſequently 3,65 meaſures of the two airs were deſtroyed; for it is $1 + 4 = 5 - 1{,}35 = 3{,}65$, and the reſult of this trial I ſhould write thus, $1a + 4n = 1{,}35$, or 3,65.

Or, for ſtill greater convenience in practice, as this laſt number 3,65, or $3\frac{65}{100}$ ſhews more immediately the goodneſs of the air in queſtion, as I have juſt obſerved, by ſuppoſing with Dr. INGEN-HOUSZ the meaſure of the eudiometer to be divided into 100 equal parts, it will be $100a + 400n = 135$, and 365, expreſſing

expreſſing the volume of the two airs deſtroyed, will become a whole number.

But, inſtead of writing $100a + 400n = 135$, &c. I ſhall continue to write $1a + 4n = 1,35$, and ſhall expreſs the laſt number (3,65) as a whole number notwithſtanding; and I ſhall ſometimes (following the example of Dr. Ingen-housz) write this number *only*, in noting the goodneſs of any air in queſtion.

I would juſt obſerve, with reſpect to the proceſs of proving the goodneſs of any kind of air, by the teſt of nitrous air, that I mix the two airs in a phial, about 1 inch in diameter and 4 inches long, putting the air to be proved into the phial firſt, and then introducing the nitrous air, one meaſure after another, till the volume of the two airs after the diminution has taken place, amounts to more than *one* meaſure, and is leſs than *two* meaſures.

Immediately after the introduction of each meaſure of nitrous air, I give the phial a couple of ſhakes; after which I ſuffer it to ſtand at reſt, while I prepare another meaſure of nitrous air, which commonly takes up about 20 ſeconds.

The meaſure of the eudiometer being filled with air, I ſuffer it to remain quiet under water 15 ſeconds, or while I can leiſurely count 30, in order that the air may have time to acquire the temperature of the water in the trough, and that the water in the meaſure may have time to run down from the ſides of the glaſs tube; and in ſhutting the ſlider I take care to bring it to be exactly even with the ſurface of the water in the trough. Similar precautions are likewiſe made uſe of in meaſuring the volume of the two airs in the tube of the eudiometer, after they have been mixed and diminiſhed in the phial.

In order that I may know when I have added nitrous air enough to the air in the phial, ſo that the volume of the two

airs may amount to 1 meaſure, and may not be greater than 2 meaſures, there are two marks upon the phial, made with the point of a diamond, the one ſhewing 1 meaſure of my eudiometer, the other ſhewing 2 meaſures.

The tube of my eudiometer is half an inch in diameter internally, and 1 meaſure occupies 3¼ inches in length upon it, and the meaſure itſelf is made of a piece of the ſame tube. Both the one and the other are ground with fine emery on the inſide, in order to take off the poliſh of the glaſs, and by that means facilitate the running down of the water, which might otherwiſe hang in drops upon the inſide of the tube upon the introduction of air.

The nitrous air was always freſh made, and of the ſame materials, *viz.* fine copper wire diſſolved in ſmoking ſpirits of nitre, diluted with 5 times its volume of water; and all poſſible attention was paid to every other circumſtance that could contribute to the accuracy of the experiments.

I have thought it neceſſary to mention theſe particulars on account of the great difference in the apparent goodneſs of any kind of air proved by the teſt of nitrous air, which ariſes from the difference of the circumſtances under which the experiments are made.

But to return to my experiments upon the air produced upon expoſing ſilk in water to the action of the ſun's rays.

Experiment N° 2.

Finding that the quantity and the quality of the air produced depended, in a great meaſure, upon the intenſity of the light by which the water and the ſilk were illuminated, I was deſirous of ſeeing whether by depriving them intirely of all light, they would not at the ſame time be deprived of the power of

furniſhing

furniſhing air. To aſcertain this fact, I took a globe A, ſimilar to that made uſe of in the foregoing experiment, and having filled it with freſh ſpring water, I introduced into it 30 grains of raw ſilk, and placing it with its cylindrical neck inverted in a jar filled with the ſame water, I covered the whole with a large inverted earthen veſſel, and expoſed it, ſo covered up, for ſeveral days in my window, by the ſide of another globe B, containing a like quantity of water and ſilk, which I left naked, and conſequently expoſed to the direct rays of the ſun.

The reſult of this experiment was, that the water and ſilk in the globe expoſed to the ſun's rays furniſhed air in great abundance, as in the experiment before-mentioned; while that in the globe covered up in darkneſs, produced only a few very inconſiderable air-bubbles, which remained attached to the ſilk.

Experiment N° 3.

To ſee if heat would not facilitate the production of air in the globe ſheltered from the light, I now removed it from the window, and placed it near a German ſtove, where I kept it warmed to about 90° of FAHRENHEIT's thermometer for more than 24 hours; but this was all to no purpoſe. The air produced was ſo exceedingly ſmall in quantity, that it could neither be proved, nor meaſured, there being only a few detached air-bubbles, which had collected themſelves near the top of the globe.

The medium heat of the water in the globe expoſed in the ſun's rays, at the time when it furniſhed air in the greateſt abundance, was about 90° FAHRENHEIT. It was ſometimes as high as 96°; but air was frequently produced in conſiderable quantities when the heat did not exceed 65° and 70°.

Experiment N° 4.

Finding by the laſt experiment (N° 3.) that heat alone, without light, was not ſufficient to enable ſilk in water to produce air, I was deſirous of ſeeing the effect of light, without heat, upon them. To this end, I took the globe B, with its contents, and plunging it into a mixture of ice and water brought it to the temperature of about 50° F. and taking it out of this mixture, and expoſing it immediately in the ſun's rays (which were very piercing at the time) I entertained it in this temperature above two hours by the occaſional application of cloths, wet in ice water, to the lower part of the globe.

Notwithſtanding this degree of cold, a conſiderable quantity of air was produced; though it was not furniſhed in ſo great abundance as when the globe was ſuffered to become hot in the ſun's rays.

Having thus aſcertained the great effect of the ſun's rays in the production of the air furniſhed upon expoſing ſilk in water to their influence, my next attempt was to determine, whether this aroſe from any peculiar quality in the ſun's light; or whether *other light* would not produce the ſame effect. With a view to aſcertaining this point, which I conceived to be of very great importance, I made the following intereſting experiment.

Experiment N° 5.

Having removed all the air from the globe B, and having ſupplied its place with a quantity of freſh water, ſo as to render it quite full, I replaced it inverted in its jar, and removing it into a dark room, ſurrounded it with 6 lamps with reflectors,

and

and 6 wax-candles, placed at different diſtances from 3 to 6 inches from it, and ſo diſpoſed as to throw the greateſt quantity of light poſſible upon the ſilk in the water, taking care at the ſame time that the water ſhould not acquire a greater heat than that of about 90° F.

Things had not remained in this ſituation above 10 minutes, when I plainly diſcovered the air-bubbles beginning to make their appearance upon the ſurface of the ſilk; and at the end of 6 hours, there was collected at the upper part of the globe a quantity of air ſufficient to make a proof of its goodneſs with nitrous air; and, upon trial, I had the pleaſure to find, that it was *dephlogiſticated*, and of ſuch a degree of purity, that 1 meaſure of it with 3 meaſures of nitrous air occupied no more than 1,68 meaſure.

I afterwards expoſed, to the ſame light, in ſmall inverted glaſs jars, filled with water, a freſh-gathered healthy leaf of the peach tree, and a ſtem of the pea plant with 3 leaves upon it; and both theſe vegetables furniſhed air in the ſame manner as they are known to furniſh it when expoſed, under ſimilar circumſtances, to the action of the ſun's direct rays, but in leſs quantities, which I attribute to the greater intenſity of the ſun's light above that of my lamps.

The experiment with the ſilk and water I repeated ſeveral times, always with nearly the ſame reſult. The quantity of air furniſhed was ſometimes a little greater, and ſometimes a little leſs; but it was always in much greater abundance than that furniſhed by an equal quantity of water and ſilk expoſed to the ſame heat, but excluded from the light; and I have reaſon to think, it was of a much ſuperior quality, though the quantity of that produced in the dark was too ſmall to be ſubmitted to any probf.

These experiments appear to me to be of so much importance, that I could wish they might be repeated, and varied in such a manner as thoroughly to establish the facts relative to the subject in question. For my part, I would most readily undertake the investigation of the matter; but being employed in another pursuit (the continuation of my Experiments upon Heat), and, besides this, much of my time being taken up by the duties of my civil employment, I have not leisure, at present, for such an undertaking.

Perhaps it may be proved by future experiment, that the matter of light is a constituent part of what is called pure or dephlogisticated air; if so, may we not venture to conclude with M. SCHEELE, that the *light*, as well as the heat, produced by flame, and in general all burning bodies, arises *solely* from the decomposition of this air, and not from the phlogiston or inflammable principle of the body which is burnt? There are many phænomena which would seem to justify this opinion.

But to proceed in the account of my experiments.—The operation of inverting the globes under water, and placing them in the jars, and of displacing and replacing them upon removing the air produced, being attended with some inconveniences, I had recourse to another method of disposing of the apparatus, much more simple and more convenient. The globes being filled were laid upon a small piece of deal board, with their necks inclined at an angle of about 20° above the plane of the horizon, and supported in this position by a perpendicular fork of wood, fixed to the end of the board, as represented by the figure. (See Tab. VI.) The part of the board, upon which the under part of the globe reposed, was hollowed a little, to prevent the globe from rolling; or, what I found

more

more safe and convenient, a small ring, or hoop, of soft wood was nailed down upon the board in that part.

By this arrangement the jars were spared, and the end of the neck of the globe being easy to be come at, by introducing a wire, or, what I commonly made use of in preference, a small glass tube, into the globe, the air hanging attached to the silk can at all times be separated from it; which is often necessary, in order to determine with greater precision the quantity of air furnished in any given time.

The air produced naturally rises to that part of the globe which is uppermost, where it collects in a body, driving out an equal volume of water; which, to prevent its running about, may be collected, by placing a proper vessel under the mouth, or end of the neck of the globe, to receive it.

The method of removing the air from the globe is too well known to require a description. I would however observe, that in doing it care should be taken, that the water in which the globe is immersed be quite clean, and of the same kind with that in the globe, otherwise that which enters the globe, to replace the air removed, might derange the experiment.

Having provided myself with a number of globes of different sizes, all fitted with boards or stands to support them, in the manner above described, I proceeded in the course of my experiments as follows.

Finding that raw silk, exposed in water to the action of light, causes the water to yield pure air in so great abundance, I was desirous of finding out whether this arose from any peculiar quality possessed by the silk; or whether other bodies might not be made to produce the same effect: to this end, having provided 6 globes, each about 4½ inches in diameter, and having filled them with fresh spring-water, I introduced into them

them the following ſubſtances, and expoſed them all, at the ſame time, to the action of the ſun's rays.

In the globe N° 1. I put 15 grains of ſheep's wool,
N° 2. —— 15 grains of Eider down,
N° 3. —— 15 grains of the fine fur of a Ruſſian hare,
N° 4. —— 15 grains of cotton wool,
N° 5. —— 15 grains of lint, or the ravelings of fine linen,
N° 6. —— 15 grains of human hair; theſe ſubſtances being all well waſhed, and being thoroughly freed of air, by being wet before they were put into the globes.

The reſults of theſe experiments were as follows.

Experiment N° 6.

The globe N° 1. which contained the ſheep's wool, did not begin to furniſh air in any conſiderable quantity till the third day of its being expoſed to the action of the ſun's rays; and ſeveral days of cloudy weather intervening, I did not remove the air till the eighth day, when I collected 1¼ cubic inch, which, proved with nitrous air, gave $1a + 3n = 1{,}28$, or 272 degrees.

The wool at no time furniſhed more than one-third part of the air, which an equal quantity of ſilk would have furniſhed under the ſame circumſtances.

The water was very faintly tinged of a greeniſh hue.

Experiment N° 7.

The water in the globe N° 2. with the Eider down, began almoſt immediately to furniſh air, and continued to yield it during the whole time of the experiment, nearly in as large quantities

quantities as the water with silk had done in the former experiments, and nearly of the same quality. 1¼ cubic inches of this air, furnished the eighth day from the beginning of the experiment, or the third of sunshine, proved with nitrous air gave $1a+3n=1{,}34$, or 266 degrees of purity.

The water was faintly tinged of a greenish, yellowish cast, and the Eider down, when examined attentively, appeared to be covered with a greenish slime.

Experiment N° 8.

The globe N° 3. with the hare's fur (which was white) furnished more air than the sheep's wool, but not so much as the Eider down. After four days of sunshine, I collected 2 cubic inches of this air, which, proved with nitrous air, gave $1a+3n=1{,}44$, or 256.

The water had acquired a very faint yellowish hue; but it did not appear to have lost much of its transparency, or to be disposed to deposit any sediment.

The air produced in this experiment made its appearance in a different manner from that furnished in those preceding it, the air-bubbles which appeared upon the surface of the fur being at considerable distances from each other, and growing to an uncommon size before they detached themselves to rise to the surface of the water.

Experiment N° 9.

The globe N° 4. with cotton wool furnished a considerable quantity of air which appeared to be better than that furnished by any of the five other globes. Proved with nitrous air, it turned out $1a+3n=1{,}07$, or 293; and, what was particular, the water did not appear to have altered its colour in the least, or to have lost any thing of its transparency.

Experiment N° 10.

The globe N° 5. with ravelings of linen, was very tardy in furniſhing air, and produced but a ſmall quantity; at the end of a fortnight, however, I collected about 2 cubic inches, which, proved with nitrous air, gave $1a + 3n = 1,51$, or 249.

The air appeared to have very little diſpoſition to fix itſelf to the ſurface of this ſubſtance. It was very ſeldom that there were air-bubbles enough attracted to it to cauſe it to riſe to the ſurface of the water, and the few bubbles which occaſionally made their appearance very ſoon diſappeared upon the diminution of the light and heat of the ſun. In ſhort, it appeared, that there is but a very feeble attraction between this ſubſtance and the particles of air, at leaſt when they are diſſolved in water. Whether this ariſes from the ſuperior affinity of the ſubſtance to water, or not, I will not pretend to decide; but it appears to be probable, as there is ſo ſtrong an attraction between water and linen, or flax, which is apparent from the avidity with which a piece of dry linen drinks up that fluid, and becomes wet, even to a conſiderable diſtance, when one end of it only is placed in it.

You will recollect that I here conſider the ſeparation of the air from water as a ſimple operation; and that I do not take into the account the purification, or rather the generation, of this air. Though there is great reaſon to conclude, that theſe two operations are very nearly connected; yet, to ſimplify my inquiries, I ſhall, in the firſt place, conſider the appearances as they preſented themſelves to my ſenſes. It will be eaſy afterwards to draw any concluſions from the reſults of the experiments, which a careful examination and compariſon of the various phænomena will juſtify.

Expe-

Experiment N° 11.

The globe N° 6. with human hair, furniſhed ſtill leſs air than that with ravelings of linen in the laſt mentioned experiment; but, notwithſtanding the ſmallneſs of the quantity, it was conſiderably ſuperior in quality to atmoſpheric air, for, proved with nitrous air, it gave $1a + 2n = 1{,}45$, or 155; whereas common air, proved at the time, gave $1a + 1n = 1{,}08$, or 92.

Experiment N° 12.

To aſcertain the relative goodneſs of the air furniſhed by the water in theſe experiments, and of that produced by expoſing freſh healthy vegetables in water to the action of the ſun's light, according to the method of Dr. INGEN-HOUSZ, I collected a ſmall quantity of air from a ſtem of a pea plant, which had four healthy leaves upon it, and found it to be much inferior to that furniſhed in the experiments with ſilk, and the various other ſubſtances I made uſe of. Proved with nitrous air, it gave $1a + 2n = 1{,}05$, or 195.

An intire plant of houſewort, of a moderate ſize, expoſed in 12 ounces of water 7 hours, to the action of the ſun's rays, at a time when the weather was remarkably fine, and very hot, furniſhed about $\frac{1}{4}$ of a cubic inch of air, which was ſo much worſe than common air, that 1 meaſure of it, with 1 meaſure of nitrous air, occupied 1,36 meaſures; or it was $1a + 1n = 1{,}36$, or 64. But I lay no kind of ſtreſs upon the reſult of this experiment, as it is more than probable, that the badneſs of the air aroſe from the roots of the plants; for from the leaves alone I have frequently ſince obtained air, which appeared to be conſiderably better than common air.

From the leaves of the peach-tree I obtained an air which, proved with nitrous air, gave $1a + 2n = 1,32$, or 168; but I did not think it neceſſary to multiply theſe experiments, particularly as Dr. INGEN-HOUSZ and Mr. SENNEBIER have given us the reſults of ſo many of theirs upon the ſame ſubject, of the accuracy of which there is no room left to doubt. I ſhall therefore content myſelf with referring to the reſults of their experiments.

With a view to determining, with greater preciſion, the quantity and the quality of the air produced by a given quantity of water and ſilk, expoſed for a given time to the action of the ſun's rays, I made the following experiment.

Experiment N° 13.

A globe of fine, clear, white glaſs, about $8\frac{5}{10}$ inches in diameter, and containing 296 cubic inches, being filled with freſh ſpring water and 30 grains of raw ſilk, was expoſed in my window three days, *viz.* the 12th, 13th, and 14th of May laſt, theſe days being for the moſt part cold and cloudy, with ſhort intervals of ſunſhine. Air produced $9\frac{1}{2}$ cubic inches; quality $1a + 3n = 1,61$, or 239.

May 15. This air being removed, and its place ſupplied with freſh water, the globe expoſed in the ſun this day from nine o'clock in the morning till five o'clock in the afternoon, the weather being very fine, yielded $8\frac{46}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,74$, or 326. The heat of the water in the globe, during the experiment, was from 70° to 98° F. The water had now loſt conſiderably of its tranſparency, and had aſſumed a light greeniſh hue.

May 16. The air furniſhed yeſterday being removed, the globe furniſhed this day, during ſix hours of ſunſhine, 9 cubic

inches of air, which, proved with nitrous air, gave $1a+4n=1{,}44$, or 356.

May 17. The globe furnished this day, during $3\frac{1}{4}$ hours of sunshine, 6 cubic inches of air, of a very eminent quality; for, proved with nitrous air, it gave $1a+4n=1{,}35$, or 365.

May 18. This day cold and cloudy; not more than $1\frac{1}{2}$ hours sunshine; air produced $\frac{1}{4}$ of a cubic inch; quality $1a+4n=1{,}56$, or 344.

May 19. The globe appearing now to be quite exhausted of air, shewing no signs of furnishing any additional quantity, though exposed to the action of a very bright sun, I removed the globe from the window, and placed it by the side of a German stove, where it was kept warm to 100° F. from 10 o'clock in the morning till 5 o'clock in the afternoon. By this means I obtained $\frac{1}{4}$ of a cubic inch of air, which, proved with nitrous air, gave $1a+4n=1{,}74$, or 326.

Not being able to obtain any more air from the globe, I now put an end to the experiment.

The quantities and qualities of the airs furnished upon the different days were as follows:

	Quantity.	Quality.
Upon the 12th, 13th, and 14th of May	$9\frac{1}{4}$ cubic inches	$1a+3n=1{,}61$, or 239
15th	$8\frac{45}{100}$	$1a+4n=1{,}74$, or 326
16th	9	$1a+4n=1{,}44$, or 356
17th	6	$1a+4n=1{,}35$, or 365
18th	$\frac{1}{4}$	$1a+4n=1{,}56$, or 344
19th	$\frac{1}{4}$	$1a+4n=1{,}74$, or 326
Total quantity	$33\frac{45}{100}$	Mean quality $1a+4n=1{,}84$, or 316

As in this experiment the air furnished each day was removed at night, and the place it occupied in the globe supplied with fresh

freſh water, I was deſirous of ſeeing what variation it would occaſion in the reſult of the experiment, if, inſtead of removing the air from time to time, I ſuffered it to remain in the globe till the end of the experiment: to this end I made

Experiment N° 14.

In which the globe being filled with freſh water, and the ſilk uſed in the laſt experiment (being firſt well waſhed), the whole was expoſed four days to the action of the ſun's rays, the weather being remarkably fine, and very hot. Upon removing the air produced, I found it amounted to $30\frac{1}{10}$ cubic inches; and its quality, proved with nitrous air, was $1a + 3n = 1{,}02$, or 298,

I ſhould have continued the experiment for ſome days longer, as the globe did not appear to be exhauſted; but the quantity of air already collected in the globe was ſo great that it became very difficult to remove it, without running the riſk of loſing a part of it, or of letting the air of the atmoſphere enter the globe, either of which would of courſe have ſpoiled the experiment. For ſafety therefore, and that I might not by an accident loſe the trouble I had already had with it, I put an end to the experiment at the end of the fourth day.

The water had loſt of its tranſparency, and had acquired a greeniſh caſt, as in the laſt experiment; and in both theſe experiments I obſerved, that a conſiderable quantity of whitiſh yellowiſh earth was precipitated by the water, which, falling to the bottom of the globe, attached itſelf to the glaſs in ſuch a manner that it was with difficulty that it could be removed. Theſe were general appearances, and took place in all caſes, in a greater or leſs degree, where a conſiderable quantity of pure air was ſeparated from water by the influence of light.

Experiment N° 15.

The ſilk made uſe of in the laſt experiment having been frequently uſed in the foregoing experiments, I was deſirous of ſeeing the effect of making uſe of freſh ſilk; and alſo of varying the proportion between the quantity of ſilk, the quantity of water, and the ſize of the globe; accordingly, at 6 o'clock P.M. upon the 13th of June, I filled a ſmall globe, about 3 inches in diameter, or (to aſcertain its ſize more exactly) which contained juſt 20 cubic inches, with freſh ſpring water, and 17 grains of raw ſilk, wound in a ſingle thread, which had never been put into water, or otherwiſe uſed, ſince it came out of the hands of the ſilk-winder.

At the end of four days, *viz.* the 14th, 15th, 16th, and 17th of June, this globe had only furniſhed ¼ of a cubic inch of air, which, proved with nitrous air, gave $1a+1n=1{,}32$, or 68; conſequently was much worſe than common air.

Upon the 18th, it began to produce good air, and during ſix hours of ſunſhine it furniſhed $1\frac{11}{100}$ cubic inches, which, proved with nitrous air, gave $1a+3n=1{,}15$, or 285.

The two following days (*viz.* the 19th and 20th of June) it furniſhed $1\frac{67}{100}$ cubic inch of air, which, proved with nitrous air, gave $1a+3n=1{,}37$, or 263; after which it totally ceaſed to yield air, though expoſed for ſeveral days in the ſun's rays.

Total quantity of air produced $2\frac{67}{100}$ cubic inches; mean quality $1a+3n=1{,}46$, or 254.

By this experiment it appears, that raw ſilk, when uſed for the firſt time, does not immediately diſpoſe the water to yield pure air; on the contrary, that it phlogiſticates the air yielded by

by water to a very conſiderable degree; and this I afterwards found to be the caſe with ſeveral other ſubſtances.

Though the quality, at a medium, of the air furniſhed in this experiment was not quite ſo good as that furniſhed in the two experiments laſt mentioned (*viz.* N° 13. and N° 14.), yet its quantity, in proportion to the quantity of water made uſe of, was greater than in either of them: it amounted to ſomething more than *one-eighth* of the volume of the water.

Of all the ſubſtances I had hitherto made uſe of in theſe experiments, raw ſilk had furniſhed the greateſt quantity of pure air; or, to expreſs myſelf more properly, had cauſed the water to furniſh the greateſt quantity; but it appeared to me very probable, that ſome other body might be found, that poſſeſſed this property in a ſtill greater degree than ſilk. Turning this matter in my mind, it occurred to me, to make the experiment with the ſilky, or rather cotton-like, ſubſtance produced by a certain ſpecies of the Poplar-tree, *Populus nigra*, very common in this country, and which, I believe, grows in England. I recollected that examining it ſome time before, with a different view (that of ſeeing if it might not be made uſe of with advantage, as a ſubſtitute for Eider down), and endeavouring to render it very dry, by expoſing it in a china plate over a chafing-diſh of hot embers, when it had acquired a certain degree of heat ſmall parcels of it quitted the plate of their own accord, and mounted up to the top of the room.

This convinced me of the time not only of its extreme fineneſs, but alſo of the ſtrong attraction which ſubſiſts between it and the particles of air; and it now occurred to me, that theſe qualities not only render it peculiarly proper as a ſubſtitute of Eider down, for confining heat, but likewiſe are properties

properties of all others the most necessary to its supplying the place of silk in the production of air, by exposing it in water to the action of the sun's rays. I therefore lost no time in making the following experiments.

Experiment N° 16.

The great globe (contents 296 cubic inches) being filled with fresh spring water, and 120 grains of poplar cotton, upon the evening of the 9th of June, and being the next day, the 10th of June, exposed to the sun about four hours, upon the morning of the 11th the air produced was removed, and its quantity was found to be 1¼ cubic inch. Its quality was very bad, *viz.* $1a + 1n = 1{,}65$, or 35 degrees only better than thoroughly phlogisticated air.

Upon the 11th, 12th, and 13th, 1 cubic inch of air only was produced, and this appeared to be as bad as possible; for, proved with nitrous air, it gave $1a + 1n = 2$, or 0.

Upon the 14th a few air-bubbles only were furnished; but, notwithstanding these unfavourable appearances, I still continued the experiment, and my patience was amply rewarded; for the next day, the 15th, the sun being very powerful, and the weather very hot, the water changing suddenly to a greenish colour, began all at once to give good air in great abundance. In the course of the day $10\frac{33}{100}$ cubic inches were produced, which, proved with nitrous air, gave $1a + 3n = 1{,}43$, or 257.

June 16th, a very warm clear day. The globe exposed in the sun, from 8 o'clock in the morning till 5 o'clock in the afternoon, furnished $14\frac{14}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1{,}34$, or 266.

June 17th, cloudy, with intervals of sunshine. The globe with about 4 hours sun gave $7\frac{35}{100}$ cubic inches of air, of a very eminent quality, *viz.* $1a + 4n = 1,40$, or 360.

The water having by degrees lost its transparency, and having acquired a deep green colour, it broke up this day, and deposited a green sediment; after which it recovered its transparency, and became almost colourless. It continued, notwithstanding, to furnish air in considerable quantities.

June 18th, being exposed in the sun's rays from 8 o'clock in the morning till 2 o'clock in the afternoon (when the heavens became overcast), the globe yielded $6\frac{37}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,44$, or 356.

June the 19th and 20th. These two days the globe furnished no more than $3\frac{13}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,06$, or 294; after which it ceased totally to furnish air, and the colour of the water changed to a dead yellowish cast, and the cotton assumed the same hue.

The following are the quantities and qualities of the different parcels of air furnished in the course of this experiment.

	Quantity.	Quality.
Upon the 10th of June	$1\frac{3}{4}$ cubic inches	$1a + 1n = 1,65$, or 35
11th, 12th, and 13th –	1 – –	$1a + 1n = 2$, or 0
14th –	0 – –	——— ———
15th –	$10\frac{41}{100}$ –	$1a + 3n = 1,43$, or 257
16th –	$14\frac{36}{100}$ –	$1a + 3n = 1,34$, or 266
17th –	$7\frac{35}{100}$ –	$1a + 4n = 1,40$, or 360
18th –	$6\frac{37}{100}$ –	$1a + 4n = 1,44$, or 356
19th and 20th –	$3\frac{13}{100}$ –	$1a + 3n = 1,06$, or 294
Total quantity	$44\frac{1}{4}$	Mean quality $1a + 3n = 1,23$, or 277

This

This experiment was repeated, and with nearly the ſame reſult; the total quantity of air produced being 41¾ cubic inches, and its quality, at a medium, $1a+3n=1{,}26$, or 274.

To aſcertain the relative fineneſs of this poplar cotton, and the thread of raw ſilk as ſpun by the worm, in order to make an eſtimate of the ſurface of the former, I examined them both at the ſame time under an excellent microſcope, when the diameter of the cotton, that is to ſay, of a ſingle thread or fibre of it, appeared to be not more than half as great as the diameter of the ſilk, conſequently its diameter was not more than $\frac{1}{3468}$ part of an inch; for I have ſhewn, in a former letter, that the diameter of a thread of ſilk, as ſpun by the worm, is only $\frac{1}{1734}$ of an inch.

The ſpecific gravity of the cotton I found to be very nearly equal to that of water, conſequently it is to that of ſilk as 1000 to 1754; its ſurface, therefore, is to the ſurface of an equal weight of raw ſilk in the compound proportion of 2 to 1, and of 1734 to 1000; that is to ſay, as 3468 to 1000.

Now, as the ſurface of 30 grains of raw ſilk amounts to 476 ſquare inches, the ſurface of 30 grains of poplar cotton muſt amount to 1651 ſquare inches, which gives 55 ſquare inches of ſurface for each grain in weight; conſequently the ſurface of the cotton made uſe of in the foregoing experiment (N° 16.) did not amount to leſs than 6600 ſquare inches (for 120 grains, the weight of the cotton, multiplied by 55, gives 6600); an enormous ſurface indeed for a body, whoſe *ſolid contents* did not amount to quite half a cubic inch.

From hence it appears evidently, that the quantities of air furniſhed by water, in the experiments with raw ſilk, and with poplar cotton, were neither in proportion to the quantities of theſe ſubſtances made uſe of, nor to the quantities of their

surfaces. It appears likewise, from the two last experiments, that the air which is furnished in the beginning of the experiment, or when the water is first exposed to the action of the sun's rays, is neither so good, nor in so great abundance, as afterwards, at a more advanced period; and that it totally ceases to be produced after a certain time.

To ascertain, with greater precision, the qualities of the air furnished at different periods of the experiment, or rather the period when the water begins to give good air; and also to determine the relative quantities and qualities of the airs produced in the experiments with raw silk, and in those with poplar cotton, I made the following experiments.

Experiment N° 17.

A globe, about 4¼ inches in diameter, containing just 46 cubic inches, being filled in the evening with fresh spring water, and 30 grains of raw silk which had been previously washed thoroughly to free it of air and the remains of former experiments, and being exposed the next day in my window, the weather being cold and cloudy, with not more than 1 hour of sunshine, ¼ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a + 2n = 1,86$, or 114.

The two following days, the weather being clear and moderately warm, 3¼ cubic inches of air were produced, which, proved with nitrous air, gave $1a + 3n = 1,14$, or 296.

Experiment N° 18.

The globe having been again filled with fresh spring water, and the same silk which had served in the last experiment, after 2 nights, and 1 day of about 4 hours sun, it had furnished 1$\frac{3}{4}$ cubic inch of air, whose quality was $1a + 2n = 1,1\frac{3}{4}$, or 197.

The

The two following days, the weather being very fine, it furnished $3\frac{1}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,58$, or 342.

Experiment N° 19.

The globe being again filled with fresh water, and the same silk well washed, and being exposed 2 days in the sun, it gave $2\frac{1}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,67$, or 233.

Experiment N° 20.

A like globe, with fresh water, and an equal quantity of poplar cotton which had been used in former experiments, being exposed at the same time, gave $2\frac{13}{100}$ cubic inches of air, whose quality was $1a + 3n = 1,20$, or 280.

Experiment N° 21.

A small globe, contents 20 cubic inches, with 17 grains of raw silk, exposed at the same time, gave 1 cubic inch of air, which turned out $1a + 3n = 1,37$, or 263.

Experiment N° 22.

A large globe, containing 296 cubic inches, being filled with fresh water and a small quantity of *conferva rivularis*, and exposed at the same time with the three globes above-mentioned, gave $1\frac{1}{2}$ cubic inch of air, which, proved with nitrous air, gave $1a + 2n = 1,76$, or 124.

The water in this experiment was changed to a brown colour, owing, as I conceived, to the too great heat the *conferva* acquired in the sun.

These experiments were made between the 2d and the 5th of July.

Expe-

Experiment N° 23.

Surprised at the smallness of the quantity, and the inferior quality, of the air produced in the last experiment, I was induced to repeat it; accordingly, the globe being again filled with water, and a quantity of fresh *conferva rivularis* (a small handful), and being exposed to the action of the sun's rays during 3 fine days, $13\frac{24}{100}$ cubic inches of air were produced, which, proved with nitrous air, gave $1a+3n=1,54$, or 246.

At the end of the experiment the water appeared to be very faintly tinged of a greenish cast.

The two following experiments were made upon the 20th and 21st of August.

Experiment N° 24.

A globe, about 4¼ inches in diameter (contents 46 cubic inches) being filled with fresh spring water, and 30 grains of raw silk which had been used in many preceding experiments, and being exposed to the action of the sun's rays two days, in all about 8 hours of sunshine, the weather being cloudy great part of the time, $1\frac{8}{100}$ cubic inch of air was produced, which, proved with nitrous air, gave $1a+3n=1,96$, or 204.

Experiment N° 25.

At the same time an equal globe, containing fresh spring water, and about 15 grains of poplar cotton (which had likewise been used in former experiments) produced $1\frac{28}{100}$ cubic inch of air, which, proved with nitrous air, gave $1a+3n=1,40$, or 260.

The

The water in both theſe experiments had acquired a faint greeniſh caſt; but the colour of that with the cotton was rather the deepeſt.

Upon examining this water under a microſcope, I found it contained a great number of animalcules, exceedingly ſmall, and of nearly a round figure. That with the ſilk contained the ſame kind of animalcules likewiſe, but not in ſo great abundance. I never failed to find them in every caſe in which the water uſed in an experiment had acquired a greeniſh hue; and from their preſence alone, I think it more than probable, that the colour of the water, *in the firſt inſtance*, aroſe in all caſes. I have ſpent a great deal of time in obſerving them, and have made many experiments upon their production; but as I have not yet been able to ſatisfy my own mind, with reſpect to the part they act in the operation of purifying the air in water, I ſhall not add to the length of this letter by giving an account of my enquiries and obſervations reſpecting them.

I was yet by no means ſatisfied with reſpect to the part which the ſilk and other bodies, expoſed in water in the foregoing experiments, acted in the purifying or dephlogiſticating of the air produced.

Dr. PRIESTLEY has long ſince diſcovered, that many animal and vegetable ſubſtances putrefying, or rather diſſolving, in water, in the ſun, cauſe the water to yield large quantities of dephlogiſticated air; but I could hardly conceive, that the ſmall quantity of ſilk which was uſed in my experiments, and which had been conſtantly in water for more than three months, and had ſo often been waſhed, and even boiled in water, ſhould yet retain a power of communicating any thing to the large quantities of freſh water in which it was ſucceſſively placed; at leaſt *any thing* in ſufficient quantities to impregnate

nate those bodies of water, and to cause them to yield the great abundance of air which they produced.

It was still more difficult to account for the purification of the air in the experiments with wool and fur, and human hair; especially, as in some of these experiments the water had not sensibly changed colour, nor did it appear to have lost any thing of its transparency. It is true, in these cases, the quantities of air produced were very small; but yet its quality was better than that of common air, and considerably superior to that of the air existing in the water, previous to its being exposed to the action of the sun's light. In short, it was dephlogisticated in the experiment; but the *manner* in which this was done is very difficult to ascertain.

With a view to throwing some new light upon this intricate subject, I made the following experiments.

Experiment N° 26.

Concluding that if silk and other bodies, used in the foregoing experiments, actually did not contribute any thing, considered as chymical substances, in the process of the production of pure air yielded by water; but if, on the contrary, they acted merely as a mechanical aid in the *separation* of the air from the water, by affording a convenient surface for the air to attach itself to; in this case, any other body, having a large surface, and attracting air in water, might be made use of instead of silk in the experiment, and pure air would be furnished, though the body so made use of should be totally incapable of communicating *any thing whatever* to the water.

To ascertain this fact, washing the great globe (containing 296 cubic inches) perfectly clean, and filling it with fresh spring water, I introduced into it a quantity of the fine flexible

thread of glaſs, commonly called *ſpun glaſs*, ſuch as is uſed for making bruſhes for cleaning jewels, and for making a kind of artificial feather frequently ſold by the Jew pedlars. This ſpun glaſs is no other than common glaſs drawn out, when hot, into an exceeding fine thread; which thread, in conſequence of its extreme fineneſs, retains its flexibility after it has grown cold.

I made choice of this ſubſtance not only on account of its great ſurface, but alſo on account of the ſtrong attraction which is known to ſubſiſt between glaſs and air, and the impoſſibility of its communicating any thing to the water.

The reſult of the experiment was, that the globe being expoſed in the ſun, air-bubbles began almoſt immediately to make their appearance upon the ſurface of the ſpun glaſs, and in 4 hours $\frac{11}{100}$ of a cubic inch of air was collected, which, proved with nitrous air, gave $1a+1n=1,12$, or 88; after which, not a ſingle air-bubble more was produced, though the globe was expoſed a whole week in the window, during which time there were ſeveral very warm, fine, ſunſhiny days.

This experiment ſhews evidently, that ſomething more is wanting to the production of pure air by water, expoſed in the ſun, than merely a ſurface to which the air diſſolved in the water can attach itſelf, in order to its making its eſcape.

The air furniſhed in this experiment was doubtleſs merely that with which the water iſſuing from the earth was overcharged, and which would have made its eſcape from the water, had the water, inſtead of being expoſed with the ſpun glaſs in the ſun, been ſimply left for ſome time expoſed to the free air of the atmoſphere.

It appears, that this air, naturally exiſting in ſpring water, inſtead of being dephlogiſticated, is ſomething worſe than

common air; and this agrees with the obſervations of Dr. PRIESTLEY, and ſeems to juſtify his opinion with reſpect to the cauſe of the fertility of lands waſhed by waters iſſuing from the earth.

If the above experiment ſhews that ſomething is wanted to be mixed with water, in order to enable it to yield pure air, when expoſed to the action of the ſun's light, the following ſhew, that this *ſomething*, whatever it may be, is frequently to be found in the water itſelf, in its natural ſtate.

Experiment N° 27.

A large jar of clear white glaſs, containing 455 cubic inches, being waſhed very clean, was filled with freſh ſpring water, and inverted in a glaſs baſon of the ſame, and placed in the middle of the garden of the Elector's Palace, where it was left expoſed to the weather 28 days.

At the ſame time another like jar was filled with water, taken from a pond in the garden, in which many aquatic plants were growing, and was expoſed in the ſame place, and during the ſame period. This water had a very faint greeniſh caſt. The pond from which it was taken is fed by a large river (the Iſar), which runs by the town.

The ſecond day after theſe waters had been expoſed in the ſun, I obſerved, that a ſmall quantity of air had collected itſelf at the upper part of each of the jars.

The third, fourth, and fifth days, the pond water furniſhed air in pretty large quantities; and it went on to yield it without intermiſſion, when the ſun ſhone upon it, till the fourteenth day, when it ſeemed to be nearly exhauſted. I continued the experiment, however, till the twenty-eighth day, though during the

the last fortnight the quantity of air in the jar did not appear to be sensibly increased.

The spring water, during the first five or six days, furnished very little air; and it was not till the fourteenth day that it began to yield it in any considerable quantities. From this time it went on to furnish it, though but very slowly, till about the twenty-second day, when it ceased, appearing to be quite exhausted.

Upon the twenty-eighth day I removed the airs from the jars, when I found their quantities and qualities to be as follows:

	Quantity.	Quality.
Air furnished by the spring water	14 cubic inches	$1a+2n=1,62$, or 138
by the pond water	$31\frac{1}{2}$ -	$1a+3n=1,78$, or 252

Neither the colour of the spring water, nor that of the pond water, appeared to be sensibly changed; but both the one and the other of these waters had deposited a considerable quantity of earth, which was found adhering to the surfaces of the glass basons in which the jars were inverted.

As these basons were rather deep, and as they were very thick in glass, and consequently not very transparent, their bottoms, where the sediment of the water was collected, were, in a great measure, obscured or deprived of the direct rays of the sun. Suspecting that this circumstance might have had some effect, so as to have hindered the water from furnishing so much air as otherwise it might have yielded, to satisfy myself respecting this matter I repeated the experiment, disposing the apparatus in such a manner, that the sediment of the water, which attached itself to the bottom of the vessel in which the jar was inverted, had the advantage of being perfectly illuminated.

Experiment N° 28.

In a large cylindrical jar, of very fine tranſparent glaſs, 10 inches in diameter, and 12 inches high, filled with ſpring water, I inverted a conical glaſs jar, 9¼ inches in diameter at the bottom, and containing 344 cubic inches, filled with the ſame water; and expoſed the whole 21 days, in a window fronting the ſouth.

The quantity of air produced amounted to 40 cubic inches; and its quality, proved by the teſt of nitrous air, gave $1a + 3n = 1{,}87$, or 2⅛.

The water in this experiment furniſhed very little air till the ſeventh day; but after that time, having aſſumed a faint greeniſh caſt, and a fine greeniſh ſlimy ſediment (the *green matter* of Dr. PRIESTLEY) beginning to be formed upon the bottom of the jar, it began to yield air in abundance, and continued to furniſh it in pretty large quantities till about the eighteenth day, when it appeared to be exhauſted.

Why the water ſhould turn green in this experiment, and not in the laſt, I know not; unleſs it was in conſequence of the large ſurface of water in the cylindrical jar, which was expoſed to the air in this experiment; or in conſequence of the ſun's ſhining directly upon the bottom of the veſſel where the ſediment was formed.

In the former experiment the baſon in which the jar was inverted was but juſt big enough to admit the jar; and as the jar was cylindrical, the ſurface of the water expoſed to the atmoſphere, in the baſon, was but very ſmall; and the baſon being very thick, and formed of glaſs which, though of the white kind, was of an inferior quality, and very imperfectly tranſparent, as I have already obſerved, the bottom of the baſon,

where the ſediment was formed, was but very imperfectly illuminated.

I intended to have repeated theſe experiments with variations, and to have made ſeveral others which I had projected, and which I thought might have thrown ſome further light upon this wonderful proceſs of the production of the pure air yielded by water; but a ſeries of unfavourable weather putting a ſtop to my enquiries, and my time having been much taken up ſince with other avocations, I have hitherto been prevented from putting my deſigns in execution; and the ſeaſon proper for theſe experiments is now ſo far advanced, that I do not think it will be in my power to recommence them till the next year. In the mean time, to fulfil my promiſe to you, I ſend you this account of the progreſs I have already made in theſe reſearches; and, when I ſhall find leiſure to purſue the matter further, I ſhall not fail to acquaint you with the reſult of my enquiries.

I have the honour to be, &c.

POSTSCRIPT.

SINCE writing the above, an interval of fine weather, and a moment of leiſure, have given me an opportunity of making a few more experiments, of which I have thought it right to give you a ſhort account.

And I muſt begin by acquainting you, that having never been thoroughly ſatisfied with reſpect to the origin of the dephlogiſticated air produced upon expoſing freſh vegetables in water to the action of the ſun's rays, according to the method of Dr. INGEN-HOUSZ, my doubts, with reſpect to the opinion generally entertained of its being *elaborated* in the veſſels of the plant,

plant, instead of being removed, were rather confirmed by the result of the experiments of which I have given an account in the foregoing letter; and however disposed I was to adopt the beautiful theory of the purification of the atmosphere by the vegetable kingdom, I was not willing to admit a fact which has been brought in support of it, till it should appear to me to have been demonstrated by the most decisive experiments.

That the fresh leaves of certain vegetables, exposed in water to the action of the sun's rays, cause a certain quantity of pure air to be produced, is a fact which has been put beyond all doubt; but it does not appear to me to be by any means so clearly proved, that this air is "*elaborated* in the plant by the "powers of vegetation;"—"phlogisticated or fixed air being "first absorbed or imbibed by the plant as food, and the dephlo-"gisticated air being rejected as an excrement:" for, besides that many other substances, and in which no elaboration, or circulation, can possibly be suspected to take place, cause the water in which they are exposed to the action of light to yield dephlogisticated air as well as plants, and even in much greater quantities, and of a more eminent quality, the circumstances of the leaves of a vegetable, which, accustomed to grow in air, are separated from its stem, and confined in water, are so unnatural, that I cannot conceive, that they can perform the same functions in such different situations.

Among many facts which have been brought in support of the received opinion of the *elaboration* of the air in the vessels of the plants in the experiments in question, there is one upon which great stress has been laid, which, I think, requires further examination.

The fresh healthy leaves of vegetables, separated from the plant, and exposed in water to the action of the sun's rays,

appear, by all the experiments which have hitherto been made, to furniſh air *only for a ſhort time*; after a day or two, the leaves changing colour, ceaſe to yield air: and this has been conceived to ariſe from the powers of vegetation being deſtroyed; or, in other words, the death of the plant; and from hence it has been inferred, with ſome degree of plauſibility, not only that the leaves actually retained their vegetative powers for ſome time after they were ſeparated from their ſtock, but that it was in conſequence of the exertion of theſe powers, that the air, yielded in the experiment, was produced.

But I have found, that though the leaves, expoſed in water to the action of light, actually do ceaſe to furniſh air, after a certain time, yet that they *regain* this power after a ſhort interval, when they furniſh (or rather cauſe the water to furniſh) more and better air than at firſt, which can hardly be accounted for upon the ſuppoſition that the air is *elaborated* in the veſſels of the plant.

Experiment N° 29.

A globe, containing 46 cubic inches, filled with freſh ſpring water and two peach leaves, was expoſed in the window to the action of the ſun's rays, 10 days ſucceſſively (the weather being in general fine), when the following appearances took place.

The 1ſt and 2d day, a certain quantity of air was produced, about as much as in former like experiments. The 3d day very little was produced; and the 4th day none at all, the globe to all appearance being quite exhauſted. Continuing the experiment, however, upon the 5th day, the water having acquired a faint greeniſh hue, air was again produced pretty plentifully, *making its appearance upon the ſurface of the leaves in the form of air-bubbles*,

bubbles, as at the beginning of the experiment; at the end of the 6th day the air was removed, and it was found to amount to $\frac{1}{100}$ of a cubic inch, its quality being 232 degrees, or $1a + 3n = 1{,}68$.

Upon the 7th day $\frac{2}{100}$ of a cubic inch of air was produced of 297 degrees, or $1a + 3n = 1{,}63$; and

During the 8th, 9th, and 10th days, 1 cubic inch of air, of 307 degrees (or $1a + 4n = 1{,}93$), was furnished; after which an end was put to the experiment.

Total quantity of air produced $3\frac{13}{100}$ cubic inches; mean quality 291 degrees, or $1a + 3n = 1{,}09$.

Finding that leaves which were dead, or in which all the powers of vegetation were evidently deſtroyed, continued notwithſtanding to ſeparate air from water, and that in ſo great abundance, I was deſirous of ſeeing the effect of expoſing freſh healthy leaves in water which I knew to be previouſly ſaturated with, and diſpoſed to yield dephlogiſticated air. I conceived, that if the plants expoſed in water actually imbibed fixed or phlogiſticated air as food, and, after digeſting it, "diſcharged "the dephlogiſticated air as an excrement;" in that caſe, as there is no inſtance of any plant, or animal, being able to nouriſh itſelf with its own excrement, the leaves expoſed in water ſaturated with dephlogiſticated air, inſtead of imbibing and elaborating it, would immediately die.

The experiments which I made to aſcertain this fact, and which, without any comment, I ſhall ſubmit to your conſideration, were as follows.

Experiment N° 30.

Having provided a quantity of water, which, by being expoſed with a few green leaves in the ſun, had acquired a greeniſh caſt, and which I found was diſpoſed to yield dephlogiſticated

gisticated air in great abundance, I filled a globe, containing 46 cubic inches, with this water, and putting to it two healthy peach leaves, expoſed the globe in the ſun upon the 7th of September, from 11 o'clock in the morning till 2 o'clock in the afternoon (3 hours), when $\frac{7}{100}$ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a + 3n = 1{,}52$, or 248 degrees.

A like globe, with freſh ſpring water and two peach leaves, expoſed at the ſame time, furniſhed only $\frac{1}{9}$ of a cubic inch of air, which, on account of the ſmallneſs of its quantity, I did not ſubmit to the teſt of nitrous air.

Experiment N° 31.

September 8. Very fine clear weather, but rather cold for the ſeaſon. Three equal globes, A, B, and C, containing each 46 cubic inches, were filled as follows, and expoſed in the ſun from 9 o'clock in the morning till half an hour paſt 4 in the afternoon, when they were found to have produced air as under mentioned.

The globe A, filled with water, which, by being previouſly expoſed in the ſun for ſeveral days, with potatoes cut in thin ſlices, had turned green, furniſhed $\frac{9}{100}$ of a cubic inch of air of 299 degrees, or $1a + 3n = 1{,}01$. N. B. This water, before it was put into the globe, was ſtrained through two thickneſſes of very fine Iriſh linen.

The globe B, filled with the ſame green potatoe water (ſtrained as before) to which were added four middling-ſized peach leaves, furniſhed $2\frac{1}{4}$ cubic inches of air of 320 degrees, or $1a + 4n = 1{,}80$.

The globe C, filled with freſh ſpring water, with four peach leaves, furniſhed $\frac{15}{100}$ of a cubic inch of air of 151 degrees, or which, proved with nitrous air, gave $1a + 2n = 1{,}49$.

To aſcertain the quantities and qualities of the airs remaining in the different waters uſed in this experiment, putting the globes ſeparately over a chafing-diſh of live coals, and making the water boil, taking care to hold the globe in ſuch an inclined poſition as that the air ſeparated from the water might be collected in the upper part of the globe, the airs produced were as follows.

	Quantity.	Quality.
By the green water in the globe A,	$\frac{1}{10}$ of a cubic inch	280 degrees
By the green water in the globe B,	$\frac{1}{160}$ — —	241
By the ſpring water in the globe C,	$\frac{1}{160}$ — —	68

The waters in theſe experiments were made to boil but for a moment; otherwiſe, it is probable, more air might have been ſeparated from them.

Finding that freſh leaves, expoſed to the action of the ſun's rays, in water which had already turned green, cauſed pure air to be ſeparated from the water in ſo great abundance, I repeated the experiment, only, inſtead of leaves, I now made uſe of a ſmall quantity of *conferva rivularis*; when I had nearly the ſame reſult as with the leaves.

To aſcertain the relative quantities and qualities of the airs yielded by the green water, when expoſed with freſh leaves, and when expoſed with raw ſilk; and alſo to aſcertain, at the ſame time, how long leaves, expoſed in green water, retain their power of ſeparating air from it, I made,

Experiment N° 32.

Two equal globes, A and B (containing 46 cubic inches), the former (A) filled with green potatoe water, ſtrained through linen, and four peach leaves; the latter (B) filled with the ſame potatoe water, ſtrained in like manner, and 17 grains of

raw silk, were exposed from Sunday noon, September 10th, till Monday evening, the weather being cold, with many flying clouds, in all about 6 or 7 hours sun.

The airs produced were as follows.

	Quantity.	Quality.
By the globe A, with green water and 4 peach leaves	$2\frac{7}{10}$ cubic inches	292 deg.
By the globe B, with green water and 17 grs. of raw silk	$2\frac{7}{10}$ – –	307

Another globe (C), filled with green water *alone*, was exposed at the same time; but it was broken by an accident before the experiment was completed.

The two globes (A and B) with their contents, being again exposed from Tuesday noon till Thursday evening, yielded air as follows.

	Quantity,	Quality.
The globe A, with the peach leaves	$4\frac{7}{10}$ cubic inches	344 degrees
The globe B, with raw silk	$4\frac{7}{10}$ – –	350

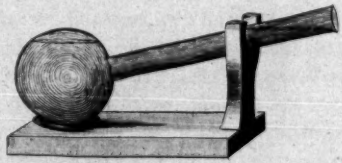
N. B. The weather on Tuesday and Wednesday was cold, with very little sunshine; but Thursday was a very fine warm day, when the greatest part of the air was produced. This air was removed and proved on Friday morning the 15th September.

Perhaps all the appearances above described might be satisfactorily accounted for, by supposing the air produced in the different experiments to have been generated in the mass of water by the *green matter*; and that the leaves, the silk, &c. did no more than *assist it in making its escape*, by affording it a convenient surface to which it could attach itself, in order to its collecting itself together, and taking upon itself its elastic form.

The phænomena might likewiſe be accounted for by ſuppoſing the *green matter* to be a vegetable ſubſtance, agreeable to the hypotheſis of Dr. PRIESTLEY, and that attaching itſelf to the ſurfaces of the bodies expoſed in the water, as a plant is attached to its ſoil, it grows; and, in conſequence of the exertion of its vegetative powers, the air yielded in the experiment is produced.

I ſhould moſt readily have adopted this opinion, had not a moſt careful and attentive examination of the green water, under a moſt excellent microſcope, at the time when it appeared to be moſt diſpoſed to yield pure air in abundance, convinced me, that, *at that period*, it contains nothing that can poſſibly be ſuppoſed to be of a vegetable nature. The colouring matter of the water is evidently of an animal nature, being nothing more than the aſſemblage of an infinite number of very ſmall, active, oval-formed animalcules, without any thing reſembling *tremella*, or that kind of *green matter*, or water moſs, which forms upon the bottom and ſides of the veſſel when this water is ſuffered to remain in it for a conſiderable time, and into which Dr. INGEN-HOUSZ ſuppoſes the animalcules above-mentioned to be actually transformed.

But having finiſhed the account of my experiments, I ſhall finiſh my letter, not daring to venture conjectures upon a ſubject ſo intricate in itſelf, and which is yet ſo new, and upon which the ableſt philoſophers of the age ſeem to be ſo much divided in opinion.



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XVI. *An Account of the Diſcovery of Two Satellites revolving round the* Georgian *Planet. By* William Herſchel, *LL.D. F. R. S.*

Read Feb. 15, 1787.

THE great diſtance of the Georgian planet, and its preſent ſituation in a part of the zodiac which is ſcattered over with a multitude of ſmall ſtars, has rendered it uncommonly difficult to determine whether, like Jupiter and Saturn, it be attended by ſatellites. In purſuit of this inquiry, having frequently directed large teleſcopes to this remote planet, and finding myſelf continually diſappointed, I aſcribed my failure to the want of ſufficient light in the inſtruments I uſed; and, for a while, gave over the attempt.

In the beginning of laſt month, however, I was often ſurpriſed when I reviewed nebulæ that had been ſeen in former *ſweeps*, to find how much brighter they appeared, and with how much greater facility I ſaw them. The cauſe of it could be no other than the quantity of light that was gained by laying aſide the ſmall ſpeculum, and introducing the *Front-view*; an account of which has been inſerted, by way of note, to the Catalogue of Nebulæ contained in the Philoſophical Tranſactions, vol. LXXVI. p. 499.

It would not have been pardonable to neglect ſuch an advantage, when there was a particular object in view, where an acceſſion of light was of the utmoſt conſequence; and I wondered

dered why it had not ſtruck me ſooner. The 11th of January, therefore, in the courſe of my general review of the heavens, I ſelected a *ſweep* which led to the Georgian planet; and, while it paſſed the meridian, I perceived near its diſk, and within a few of its diameters, ſome very faint ſtars whoſe places I noted down with great care.

The next day, when the planet returned to the meridian, I looked with a moſt ſcrutinizing eye for my ſmall ſtars, and perceived that two of them were miſſing. Had I been leſs acquainted with optical deceptions, I ſhould immediately have announced the exiſtence of one or more ſatellites to our new planet; but it was neceſſary that I ſhould have no doubts. The leaſt hazineſs, otherwiſe imperceptible, may often obſcure ſmall ſtars; and I judged, therefore, that nothing leſs than a ſeries of obſervations ought to ſatisfy me, in a caſe of this importance. To this end I noticed all the ſmall ſtars that were near the planet the 14th, 17th, 18th, and 24th of January, and the 4th and 5th of February; and though, at the end of this time, I had no longer any doubt of the exiſtence of at leaſt one ſatellite, I thought it right to defer this communication till I could have an opportunity of ſeeing it actually in motion. Accordingly I began to purſue this ſatellite on Feb. the 7th, about ſix o'clock in the evening, and kept it in view till three in the morning on Feb. the 8th; at which time, on account of the ſituation of my houſe, which intercepts a view of part of the ecliptic, I was obliged to give over the chace: and during thoſe nine hours I ſaw this ſatellite faithfully attend its primary planet, and at the ſame time keep on, in its own courſe, by deſcribing a conſiderable arch of its proper orbit.

While I was chiefly attending to the motion of this ſatellite, I did not forget to follow another ſmall ſtar, which I was pretty

pretty well assured was also a satellite, especially as I had, on the night of the 14th of January, observed two small stars which were wanting the 17th, and again missed other two the 24th which had been noticed the 18th; but, whether owing to my great attention to the former satellite, or to the closeness of this latter, which was nearly hidden in the rays of the planet, I could not be well assured of its motion. Indeed, towards morning, when a change of place, in so considerable an interval as nine hours, would have been most conspicuous, the moon interfered with the faint light of this satellite, so that I could no longer perceive it.

The first moment that offered for continuing these observations was on February the 9th, when I saw my first discovered satellite nearly in the place where I expected to find it. I perceived also, that the next supposed satellite was not in the situation where I had left it on the 7th, and could now distinguish very plainly that it had advanced in its orbit, since that day, in the same direction with the other satellite, but at a quicker rate. Hence it is evident, that it moves in a more contracted orbit; and I shall therefore call it in future the first satellite, though last discovered, or rather last ascertained; since I do not doubt but that I saw them both, for the first time, on the same day, which was January the 11th, 1787.

I now directed all my attention to the first satellite, and had an opportunity to see it for about three hours and a quarter; during which time, as far as one might judge, it preserved its course. The interval which the cloudy weather had afforded was, however, rather too short for seeing its motion sufficiently, so that I deferred a final judgment till the 10th; and, in order to put my theory of these two satellites to a trial, I made a sketch on paper, to point out before-hand their situation.

ſituation with reſpect to the planet, and its parallel of declination.

The long expected evening came on, and, notwithſtanding the moſt unfavourable appearance of dark weather, it cleared up at laſt. And the heavens now diſplayed the original of my drawing, by ſhewing, in the ſituation I had delineated them, *The Georgian Planet attended by two Satellites.*

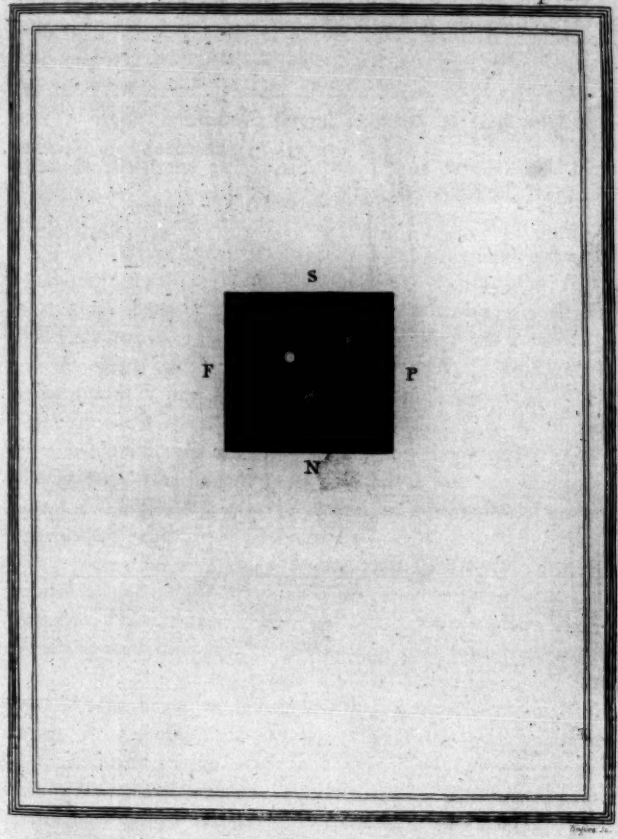
I confeſs that this ſcene appeared to me with additional beauty, as the little ſecondary planets ſeemed to give a dignity to the primary one, which raiſes it into a more conſpicuous ſituation among the great bodies of our ſolar ſyſtem.

For upwards of five hours I ſaw them go on together, each purſuing its own track; and I left them ſituated, about two o'clock in the morning on February the 11th, as they are repreſented in the figure, Tab. VII. The letters S, N, P, F, denote the ſouth, north, preceding, and following parts of the heavens, as they are ſeen, by the *front-view*, in my teleſcope. The ſouth preceding ſatellite is the ſecond, or that whoſe motion was firſt aſcertained; the other is that which moves in a ſmaller orbit, or what I have called the firſt ſatellite; and the direction of their motion is according to the order P, S, F, N, of the letters.

I have not ſeen them long enough to aſſign their periodical times with great accuracy; but ſuppoſe that the firſt performs a ſynodical revolution in about eight days and three-quarters, and the ſecond in nearly thirteen days and an half.

Their orbits make a conſiderable angle with the ecliptic; but to aſſign the real quantity of this inclination, with many other particulars, will require a great deal of attention, and much contrivance: for, as eſtimations by the eye cannot but be extremely fallacious, I do not expect to give a good account of

their


S
F
P
N

their orbits till I can bring ſome of my micrometers to bear upon them; which, theſe laſt nights, I have in vain attempted, their light being ſo feeble as not to ſuffer the leaſt illumination, and that of the planet not being ſtrong enough to render the ſmall ſilk-worm's threads of my delicate micrometers viſible. I have, nevertheleſs, ſeveral reſources in view, and do not deſpair of ſucceeding pretty well in the end.

W. HERSCHEL.

Slough, near Windſor,
February 11, 1787.

XVII. *Remarks on Mr.* Brydone's *Account of a remarkable Thunder-ſtorm in* Scotland. *By the Right Honourable* Charles *Earl* Stanhope, *F. R. S.*

Read February 15, 1787.

§ 1. NO ſtorm of lightning has ever produced effects more curious to contemplate than thoſe related by Mr. Brydone, in his Letter to the Preſident of this Society. That account contains facts of ſuch conſequence, and ſo perfectly inexplicable by the principles of electricity which are commonly received, that it undoubtedly deſerves particular attention.

It appears, that a man (named "James Lauder") ſitting on the fore-part of a cart drawn by two horſes, was ſuddenly ſtruck dead, as alſo the horſes that he was driving, and that the cart itſelf was much injured by electrical fire, although no lightning fell at, or near, the place where this accident happened.

§ 2. Before I attempt to account, by the laws of electricity, for this remarkable phænomenon, it may not be improper juſt to take notice, that few facts of this kind have ever been *better authenticated* than this is.

It appears, firſt, that a man, who was ſitting upon the fore-part of another cart, only twenty-four yards behind the cart that was ſtruck, "had Lauder, his cart and horſes, full in "view when they fell; he was ſtunned by a loud report, and "ſaw his companion, his horſes and cart, fall to the ground; "he

" he immediately ran to his aſſiſtance, but found him quite " dead; he perceived" (at the time of the accident) " no flaſh " or appearance of fire."

It alſo appears, that another man, a ſhepherd of St. Cuthbert's farm, was alſo a witneſs of this event. He was diſtant from LAUDER " between two and three hundred yards, and was " looking at the two carts, when he was ſtunned by a loud " report, and at the ſame inſtant ſaw the firſt of the carts fall " to the ground. He ſaw no lightning, nor appearance of fire " whatever."

The concurrent teſtimony of theſe two men is confirmed by PATRICK BRYDONE, Eſq. who lives " at a ſmall diſtance" from the ſpot where LAUDER was killed: and Mr. BRYDONE relates, that a ſtorm appeared far off; and that he, and ſome company in his houſe, were " ſuddenly alarmed by a loud " report, for which they were not prepared by any preceding " flaſh." There is the greater weight to be given to this account of Mr. BRYDONE, as it ſo happened, that he was juſt then " obſerving the progreſs of the ſtorm, at an open win- " dow, in the ſecond ſtory of his houſe," and making the company " obſerve, by a ſtop-watch, the time that the ſound " took to reach them."

It is extremely fortunate, that a gentleman, fond of philoſophical enquiries, ſhould have been upon the ſpot at the time of this accident, to give to the world ſo clear and intereſting an account.

§ 3. That the death of LAUDER and of the horſes was *not* occaſioned by any *direct main ſtroke of exploſion* from a thunder-cloud, either poſitively or negatively electrified, is evident; ſince no lightning whatever did paſs from the clouds to the

earth (or from the earth to the clouds) at the place where they were killed.

§ 4. It is equally evident (and for the very ſame reaſon) that they were not deprived of life by any *tranſmitted main ſtroke of exploſion*, either poſitive or negative.

§ 5. It is alſo obvious, that the *lateral exploſion* was not the cauſe of this miſchief; for the *lateral* * *exploſion* does always proceed immediately from the main ſtroke itſelf; and therefore there can exiſt no *lateral exploſion*, in the caſe when there is no main ſtroke whatever.

§ 6. It might, perhaps, be ſuppoſed, that LAUDER and the two horſes were ſuddenly ſuffocated by the ſulphureous ſmell that often accompanies electricity.

But though the death of the man and of the horſes might (by ſome) be attributed to this cauſe, the ſulphureous ſmell will no wiſe account for "many pieces of the coal having been "thrown out, to a conſiderable diſtance, all round the cart;" and for the "ſplinters" (from the wood-work of the cart) that were "thrown off in many places," as appears, by Mr. BRYDONE's account, to have been the caſe.

§ 7. Some perſons may imagine that LAUDER and the horſes might have been killed, and that the other effects above-mentioned might be accounted for, by ſome ſudden and very violent commotion in the air, which commotion the thunder might be ſuppoſed to produce, though unaccompanied with any flaſh at that place; in a manner, in ſome ſmall degree, ſimilar to the fatal wounds that ſometimes have been known to have been given by the air having been ſuddenly diſplaced

* See Dr. PRIESTLEY's excellent Paper in the ſixtieth volume of the Philoſophical Tranſactions, page 192 et ſeq. See alſo my Principles of Electricity, § 386.

by

by a cannon ball, in its paſſage through that atmoſpherical fluid, though the cannon ball itſelf had evidently neither ſtruck the perſon wounded, nor even grazed his cloaths.

The "duſt that roſe at the place" might be brought as an additional argument in favour of the opinion, that a ſudden and violent commotion of the air did occaſion the effects produced.

But ſuch an explanation (vague as it is) would not account for the "marks of fuſion" on the iron of the wheels; nor for "the hair of the horſes having been ſinged;" nor for "the ſkin of LAUDER's body having been burnt" in many places.

§ 8. I think, from the different circumſtances of this caſe, that it is clear to demonſtration, that the effects produced proceeded from electricity; and that *no electrical fire did paſs* immediately either from the clouds into the cart, &c. or from the cart, &c. into the clouds.

From "the circular holes" in the ground ("of about "twenty inches in diameter") the reſpective "centers of "which were exactly in the track of each wheel," and the correſponding "marks of fuſion" on the iron of the wheels, which marks "anſwered exactly to the center of each of the "holes;" it is evident, that the electrical fire did paſs (from the earth to the cart, or from the cart to the earth) through that part of the iron of the wheels which was in contact with the ground.

From the "ſplinters that had been thrown off, in many "places, particularly where the timber of the cart was con-"nected by nails or cramps of iron," and from the various other effects mentioned in Mr. BRYDONE's Paper, it is moreover evident, that there was a violent motion of the electrical fluid in all,

all, or (at leaſt) in different parts of the cart, and of the bodies of the man and horſes, although there were no lightning.

§ 9. Wonderful as theſe combined facts may appear, and uncommon as they certainly are in this country, they are, nevertheleſs, eaſy to be explained by means of that particular ſpecies of electrical ſhock, which I have diſtinguiſhed in my Principles * of Electricity (publiſhed in 1779) by the appellation of the "*electrical returning ſtroke:*" and although at the time I wrote that Treatiſe, I had it not in my power to produce any inſtance of perſons or animals having been killed in the very peculiar manner ſince related in Mr. BRYDONE's Paper; I did, however (from my experiments mentioned in that Book), venture to aſſert, with confidence, that †, "if "perſons be ſtrongly ſuperinduced by the electrical atmoſphere "of a cloud, they may (under circumſtances ſimilar to thoſe "explained in that Treatiſe) receive a very ſtrong ſhock, be "knocked down, or be even killed, at the inſtant that the "cloud diſcharges, with an exploſion, its electricity, whether "the lightning falls near the very place where thoſe perſons "are, or at a very conſiderable diſtance from that place, or "whether the cloud be poſitively or negatively electrified."

And I moreover ſtated that ‡, "whether the diſtance be"tween the perſon ſo circumſtanced, and the place where the "lightning falls, be fifty or an hundred yards, or one mile, or "two miles, or three miles, or more, the truth of the ge-

* See Principles of Electricity, containing divers new Theorems and Experiments, together with an Analyſis of the ſuperior Advantages of high and pointed Conductors; by CHARLES Viſcount MAHON, F. R. S. from § 202. to § 347. inclusively.

† See Principles of Electricity, § 311.

‡ See Principles of Electricity, § 313.

"neral

"neral propoſition there laid down would not be any wiſe "affected."

I have alſo explained, in that Treatiſe, how a ſtill more ſingular effect might be produced, namely, how * "an explo- "ſion, which happens in one place, may cauſe in a ſecond "place (at a very conſiderable diſtance from the firſt place) "a ſudden returning ſtroke, which may knock down, or even "kill, perſons and animals at that ſecond place; at the ſame "time that other perſons, or other animals, ſituated in a third "place, that is even immediately between the firſt place "where the lightning falls, and the ſecond place (juſt men- "tioned) where the ſhock of the returning ſtroke happens, "ſhall receive no detriment whatever."

§ 10. Having, in my Principles of Electricity †, explained at large the nature of the *returning ſtroke*, I will not treſpaſs upon the time of this Society, by repeating the account of any of the various experiments that I made, to prove the poſſible exiſtence of ſuch an electrical ſtroke; but I will, at once, apply the general laws, which I have (therein) laid down, to the particular phænomena related by Mr. Brydone.

But, before I ſpeak of the accident of Lauder, which appears to me to have been occaſioned by a *returning ſtroke*, proceeding from an *aſſemblage* of clouds, I will ſay a few words upon one or two other facts, mentioned in Mr. Brydone's account.

§ 11. Mr. Brydone informs us, that "the ſhepherd belong- "ing to the farm of Lennel-Hill was in a neighbouring field, "when he obſerved a lamb (only a few yards from him) drop- "down, although the lightning and claps of thunder were;

* See Principles of Electricity, § 314.

† See Principles of Electricity, from § 202 to § 347, inclusively.

" then, at a great diſtance from him. He ran up immediately, " but found the lamb quite dead; nor did he perceive the leaſt " convulſive motion, or ſymptom of life remaining, although, " the moment before, it appeared to be in perfect health."

This effect is ſo preciſely ſimilar to thoſe explained in my Principles of electricity, and particularly to that mentioned in ſection 328, that it is quite unneceſſary to enlarge upon it. I will only obſerve, that ſuch an electrical *returning ſtroke* as that by which this lamb was deſtroyed (namely, a returning ſtroke, which happens at a place where there is neither lightning nor thunder near) belongs to the moſt ſimple claſs of returning ſtrokes; and that it may be produced by the ſudden removal of the elaſtic electrical preſſure of the electrical atmoſphere of a *ſingle* * main cloud, as well as by that of an *aſſemblage* † of clouds.

It appears by Mr. BRYDONE's account, that the ſhepherd, who ſaw the lamb fall, was near enough to it to feel, in a ſmall degree, the electrical returning ſtroke " at the ſame " time" that the lamb dropped down.

§ 12. Mr. BRYDONE further relates, that " a woman " making hay near the banks of the river fell ſuddenly to " the ground; and called out to her companions, that ſhe had " received a violent blow on the foot, and could not imagine " from whence it came."

This blow was, unqueſtionably, the *electrical returning ſtroke*.

When a perſon, walking or ſtanding out of doors, is knocked down or killed by the *returning ſtroke*, the electrical fire muſt ruſh in, or ruſh out, as the caſe may be, through that

* See Principles of Electricity, § 312.

† See, in my Principles of Electricity, the end of the note to § 312.

perſon's

perſon's *feet* *, and through them *only*; which would not be the caſe, were the perſon to be knocked down or killed by any main ſtroke of exploſion, either poſitive or negative.

Theſe things are evident. But the manner in which LAUDER and the two horſes were killed is not quite ſo evident, though it appears to me to be very eaſily accounted for.

I will now ſtate, in what manner I conceive that the clouds may have been placed, when the effects mentioned in Mr. BRYDONE's Paper were produced.

§ 13. I muſt premiſe that, by that account, it appears, that the cloud was many miles in length; inaſmuch, as juſt before the "loud report," the lightning was at a very "great diſtance:" for Mr. BRYDONE "obſerved, by a ſtop-watch, that "the time that the ſound took to reach him was generally from "25 to 30 ſeconds."

Mr. BRYDONE relates, that the "loud report reſembled the "firing of ſeveral muſkets, ſo cloſe together, that the ear "could hardly ſeparate the ſounds, and was followed by no "rumbling noiſe like the other claps."

This deſcription indicates, that the electrical exploſion was not far diſtant; and alſo ſhews, that it was not extremely near: for, if the exploſion had been extremely near, the ear could not at all have ſeparated the ſounds.

§ 14. Now let us ſuppoſe a cloud, eight, ten, or twelve miles in length (be the ſame more or leſs) to be extended over the ſurface of the earth, in the ſituation repreſented by ABC in the figure. (See Tab. VIII.)

And let another cloud (as repreſented by DEF) be ſituated between the above-mentioned cloud and the earth.

* See Principles of Electricity, from § 322. to § 327. inclufively.

Let the two clouds be ſuppoſed to be charged (for inſtance) with the *ſame* kind of electricity, and to be both poſitive.

Let us further ſuppoſe, that the lower cloud DEF be near the earth, only a little beyond the ſtriking diſtance; and let a man, cart and horſes, be ſituated at L, under that part (E) of the cloud which is the neareſt to the earth.

Now, let us ſuppoſe this cart to be aſcending an hill, and to be in the ſituation deſcribed by Mr. BRYDONE, namely, to have "almoſt gained the higheſt part of the aſcent," and to be followed by "another cart" (M) lower down the hill.

Let us ſuppoſe alſo, that the two clouds DEF and ABC be near each other, perpendicularly over the place where the cart L was ſituate (as at DA).

And let the remote end C, of the upper cloud ABC, approach the earth, within the ſtriking diſtance, and ſuddenly diſcharge its electricity into the earth at G.

§ 15. Things being ſituated as above deſcribed, let us examine what conſequences muſt follow.

Firſt, when the upper cloud ABC diſcharges its electricity into the earth at G, the lower cloud DEF muſt immediately diſcharge its electricity into the upper cloud at the place DA, which is directly or nearly over the cart L.

This accounts for the "loud report" of thunder that was unaccompanied by lightning at L or at M. The *report* muſt be loud, from its being near; but no *lightning* could be perceived at L or M, by reaſon of the thick thunder cloud DEF being ſituated immediately between the ſpectator at M and DA, the place between the two clouds where the lightning was.

§ 16. Secondly, as the lower cloud DEF did gradually approach towards the earth at L, ſo as to come finally into the ſituation

ſituation repreſented in the figure; the earth at L muſt, of courſe, become ſuperinduced by the elaſtic electrical preſſure of the electrical atmoſphere of the thunder-cloud; which ſuperinduced elaſtic electrical preſſure muſt gradually have increaſed as the cloud came cloſer to the earth, and approached nearer to the limit of the ſtriking diſtance.

§ 17. Conſequently, if any conducting body (not having any prominent conducting points) were to be placed at L, upon the ſurface of the earth, and to be there *electrically inſulated*; then ſuch conducting body, by the laws of electricity, muſt, at its upper extremity (namely, the part neareſt to the *poſitive* cloud) become *negative*; at its lower extremity, it muſt become *poſitive*; and, at a certain intermediate point, it will be neither *plus* nor *minus*.

So that this inſulated conducting body, thus ſituated, will be in three oppoſite ſtates at one and the ſame time; that is to ſay, that it will be, at the ſame inſtant, *poſitively* electrified, *negatively* electrified, and not electrified at all.

This propoſition I have rigorouſly demonſtrated in my Treatiſe * on Electricity.

§ 18. But if this conducting body, on the ſurface of the earth, be *not inſulated* †, or be but imperfectly inſulated, then the *whole* of ſuch body (from its being immerged in the electrical atmoſphere of the *poſitive* cloud) will become *negative*; becauſe part of the electricity of this conducting body will, in this caſe, paſs into the earth. And this conducting body will become the more *negative*, as it becomes the more deeply immerged into the denſe part of the elaſtic electrical atmoſphere of the approaching thunder-cloud.

* See Principles of Electricity, from § 55 to § 74, incluſively.

† See Principles of Electricity, § 182 and § 183.

§ 19. Now, when the positive cloud DEF (in the manner above stated) comes suddenly to discharge, with an explosion, its superabundant electricity into the other cloud ABC, then the elastic electrical atmosphere of the cloud DEF will cease to exist; consequently, the electrical fluid, which had been gradually expelled into the common stock, from the conducting body situated (at L) upon the surface of the earth, must (by the sudden removal of the superinduced elastic electrical pressure of the electrical atmosphere of the thunder-cloud) suddenly return from the earth into the said conducting body, producing a violent * commotion, similar to the pungent shock of a *Leyden jar* in its sensation and effects.

This is what I call the electrical *returning* † *stroke*.

§ 20. It was by such a returning stroke that LAUDER and the horses that he was driving were killed, they having become strongly *negative* ‡ previous to the explosion.

LAUDER was "sitting" when he was struck dead; and his legs appear to have been "hanging over the fore-part of the "cart, at the time of the explosion." The returning stroke, therefore, could not issue into his body through his legs; and this accounts for the "skin of his legs" not having been at all "burnt or shrivelled," as the skin was on many other parts of his body. And it likewise shews the reason, why the "zig-"zag line" on LAUDER's skin (which was terminated at one end by the chin) "did not extend lower than the thigh."

* See Principles of Electricity, § 304 and § 603.

† See Principles of Electricity, from § 202 to § 208, inclusively, and § 387.

‡ Note, I have here supposed the thunder-cloud (and of course its electrical atmosphere) to have been *positive*; but similar effects would have been produced if the thunder-cloud had been *negative*; only, in that case, LAUDER and the horses would have been strongly positive, previous to the explosion.

§ 21. Mr. BRYDONE mentions, that "the hair of the horſes "was much ſinged over the greateſt part of their bodies; but "was moſt perceptible on the belly and legs." This effect is eaſily accounted for by the returning ſtroke; for the lower part * of the bodies of thoſe animals muſt of courſe have been more ſtrongly affected than any other part, as the electrical fire from the earth muſt ſuddenly have ruſhed into their bodies through "their legs, which had made a deep impreſſion "in the duſt."

§ 22. The various effects produced on the cart may be explained, with equal facility, by means of the returning ſtroke; and I have ſtated, in my Principles † of Electricity, "not "only in what manner perſons and animals may be deſtroyed, "but how" inanimate bodies, ſuch as "particular parts of build-"ings, may be conſiderably damaged by an electrical returning "ſtroke, namely, all thoſe parts where there is, in any kind "of conducting ſubſtance or ſubſtances (upon which a ſtrong "elaſtic electrical preſſure is ſuperinduced), any kind of elec-"trical interruption, acroſs which the returning electrical fire "might ſuddenly ſtrike, and might thereby rend and deſtroy "all the bodies that it might meet with in its paſſage."

Mr. BRYDONE relates, that "ſplinters had been thrown off "in many places, particularly where the timber of the cart was "connected by nails or cramps of iron." The electrical interruption between theſe pieces of metal, acroſs which interruption the returning electrical fire did ſuddenly ſtrike, accounts for its rending the bodies that it met with in its paſſage.

* See, in my Principles of Electricity, the note to § 594, where ſomewhat of a ſimilar effect is mentioned.

† See Principles of Electricity, § 333, § 334, and § 347.

It

It is alſo evident, that it was the electrical returning fire, that produced the "marks of fuſion" on that part of the iron of the wheels which was in contact with the ground; inaſmuch as the whole electricity, that, at the inſtant of the exploſion, returned into the cart, did enter at thoſe places.

§ 23. No perſon, the leaſt verſed in the principles of electricity, can heſitate to aſſent to the propoſition, that the *electrical returning ſtroke* muſt exiſt, under circumſtances ſimilar to thoſe explained above. But it may be objected to me, that although all the aforeſaid effects of a returning ſtroke might take place in a ſmall degree, yet thoſe effects could not have been ſufficiently powerful to have killed LAUDER, the horſes, and the lamb, or to have melted the iron of the cart-wheels; eſpecially, conſidering the ſmall quantity of electrical fluid that is contained in the body of a man, of a lamb, or of a horſe; or that is contained in any body of the ſize of a common cart; that is to ſay, conſidering the ſmall quantity of electrical fluid that could, by being diſturbed, have produced the *returning ſtroke*.

To this objection (plauſible as at firſt ſight it may appear) I conceive, I have given a complete anſwer in my Principles of Electricity, from ſection 337 to ſection 347, incluſively; and alſo from ſection 592 to ſection 606, incluſively; but it may not be improper to add a few words to what I have already ſaid upon that part of the ſubject.

§ 24. No legitimate concluſion can be drawn from premiſes that are not proved: therefore, no perſon can legitimately conclude, that the force of a returning ſtroke muſt always be weak, when produced by the diſturbed electrical fluid of a man's body, by reaſon that a man's body contains but a ſmall quantity of electricity: for, it has never been proved, that a man's

man's body does contain only a *ſmall* quantity of electrical fluid; neither is there the ſmalleſt reaſon to believe ſuch an hypotheſis, which appears, on many accounts, to be completely erroneous. And, if that hypotheſis be erroneous, the objection to the ſtrength of an electrical returning ſtroke remains perfectly unſupported by argument.

When a body is ſaid to be *plus* or *poſitive*, it ſimply means, that the body contains *more* electricity than it does in its unelectrified, that is to ſay, natural ſtate; but does not ſignify, that ſuch body is completely ſaturated * with electricity. In like manner, when a body is ſaid to be *minus* or *negative*, it only ſignifies, that the body contains *leſs* than its natural ſhare of electricity; but does not imply, that ſuch body is *completely exhauſted* of the electricity which it contains in its natural ſtate.

Now, the ſtrength of natural electricity is ſo immenſe, when compared to the very weak effects of our largeſt and beſt contrived electrical machines, that I conceive, that we cannot, by means of artificial electricity, expel, from a man's body, the thouſandth (or perhaps even the ten thouſandth) part of the electrical fluid which it contains, when in its natural ſtate.

§ 25. That hypotheſis, by which natural phænomena are eaſily accounted for, has a better claim to our attention than an oppoſite hypotheſis, which prevents thoſe phænomena from being intelligibly explained.

There is no reaſon whatever for concluding, that any electrical machine of any *given* ſize is capable of rendering a conducting body either *completely plus*, or *completely minus*; but far otherwiſe. And it would have been as logical, for any perſon, ſome years ago (when electrical machines were not brought to

* See Principles of Electricity, § 342.

their

their preſent ſtate), to have maintained, that thoſe very imperfect machines were capable of rendering a conducting body *completely poſitive*, or *completely negative*, as for us, in the year 1787, to conclude, that we (by our ſtill imperfect machines) have attained the limit of electrical exhauſtion, or condenſation.

We evidently have not, with our machines, even approached the limit of electrical ſtrength, particularly in reſpect to the *returning ſtroke*: for it is remarkable, that (by the laws of electricity *) the ſtrength of the electrical *returning ſtroke*, near the limit of the ſtriking diſtance, does increaſe in a "greater ratio," than the ſtrength of the main ſtroke from the charged body, producing the elaſtic electrical atmoſphere ſuperinduced.

§ 26. For example, let the returning ſtroke be attempted to be produced, by means of a metallic prime conductor of 20 or 21 inches in length, and of about two inches in diameter; and by means of another metallic body of equal dimenſions, placed parallel to the prime conductor, juſt out of the limit of the ſtriking diſtance; and let the prime conductor be charged, by means of one of the common glaſs globes, of leſs than nine inches in diameter.

The returning ſtroke, in this caſe, will not only be conſiderably weaker than a ſpark taken from the prime conductor, but it will be ſo extremely weak, that it can hardly be ſaid even to exiſt.

§ 27. Whereas, if the experiment be made in a manner exactly ſimilar, by means of a *large* glaſs cylinder (inſtead of the ſmall globe) and by means of a metallic prime conductor, of about three feet four inches long †, by nearly four inches and a

* See my Principles of Electricity, § 340 and § 341.

† It is better if the prime conductor, and the other metallic body, be ſtill larger.

half

half diameter *; and alſo by means of another metallic body of equal dimenſions with this prime conductor; then, there will be no kind of compariſon between the ſtrength of the *returning ſtroke* obtained out of the ſtriking diſtance of the prime conductor, and the ſtrength of the *main ſtroke* received immediately from the prime conductor, the ſharpneſs and pungency of the *returning ſtroke* being ſo much ſuperior. The returning ſtroke in this caſe is like the ſudden diſcharge of a weakly electrified *Leyden jar*, provided that due attention be paid to the four rules for obtaining a very ſtrong returning ſtroke, as laid down in ſection 307 of my Principles of Electricity.

§ 28. When I performed the experiment of the returning ſtroke, by means of a ſtill ſtronger electrical apparatus, the commotion † felt was ſimilar to that of a *Leyden jar*, ſtrongly electrified, ſuddenly diſcharged through my body; ſo that, having taken the returning ſtroke eight or ten times one morning (without having taken the *main ſtroke* a ſingle time that day), I felt a conſiderable degree of pain acroſs my cheſt during the whole evening, and a diſagreeable ſenſation in my arms and wriſts all the next day.

I have alſo found, that (by an advantageous diſpoſition of the apparatus) metal ‡ may be melted by means of the *electrical returning ſtroke*, not only entirely out of the *ſtriking diſtance*, but even without any communication with the common ſtock, although the conducting body, from which iſſued the electrical

* See Principles of Electricity, § 593, 594, and 595.

† See Principles of Electricity, § 304 and § 310.

‡ See Principles of Electricity, from § 603 to § 606, inclusively.

fluid that formed the *returning ſtroke*, had leſs than *twenty-ſeven* ſquare feet of ſurface in contact with the air.

§ 29. The fact is, that in the caſe of the returning ſtroke, it is not ſo much upon the *quantity* of electrical fluid, as upon the *velocity* * of that fluid, that the ſtrength of that ſtroke depends; therefore, the ſtrength of the *returning ſtroke* depends leſs upon the *quantity of ſurface* uſed, than upon the *ſtrength* of the electrical preſſure of the elaſtic electrical atmoſphere, ſuperinduced upon the body ſtruck, previous to the exploſion.

But, the electrical preſſure of the elaſtic electrical atmoſphere of the great thunder-cloud in Scotland muſt have been immenſe, when compared to the electrical preſſure of the elaſtic electrical atmoſphere of a metallic prime conductor, of whatſoever ſhape; conſequently, it is not at all ſurpriſing that LAUDER ſhould have been killed (or that the other effects, related by Mr. BRYDONE, ſhould have been produced) by the *returning ſtroke*; inaſmuch as it is not ſurpriſing, that *effects* ſhould be proportionate to the *cauſes* by which they are reſpectively produced.

§ 30. Mr. BRYDONE relates, that "LAUDER's cart was "higher on the bank" than the cart that followed him; which, in ſome degree, accounts for the man, ſitting on the other cart, *not* having felt the returning ſtroke. But, that is to be accounted for in another way, namely, by ſuppoſing the cloud to have been pending nearer to the earth, over the ſpot where

* See Principles of Electricity, from § 592 to § 602, incluſively; and particularly § 601.

LAUDER

LAUDER* was killed, than over the place where his companion was; for, I have ſhewn, in my Treatiſe upon Electricity †, that, in order for a perſon to receive a dangerous returning ſtroke, ſuch perſon ſhould be immerged, not merely in the electrical atmoſphere of the thunder-cloud, but in the denſe part of the cloud's electrical atmoſphere.

The fact above alluded to may alſo be accounted for in the following way, *viz.* by ſuppoſing that the ſecond cart were either better connected with the common ſtock, or that it were better inſulated, than LAUDER's cart: for, I have ſhewn, in my Principles of Electricity ‡, (what is very remarkable, namely,) that *either* of theſe two *oppoſite* circumſtances will weaken the force of a returning ſtroke prodigiouſly. Now, Mr. BRYDONE mentions, that there had been an "almoſt total want of rain "for many months." He alſo ſays, that "the ground" (at the place where LAUDER was killed) "was remarkably dry, "and of a gravelly ſoil." This ſtate of the ground was particularly adapted to the production of the electrical returning ſtroke, when produced upon the large ſcale of nature, where the elaſtic electrical preſſure is ſo powerful.

§ 31. The account which Mr. BRYDONE has given of this thunder-ſtorm in Scotland is not more curious than it is inſtructive.

* See Principles of Electricity, § 318.

† See, in my Principles of Electricity, Experiments 38, 39, and 40, from § 280 to § 296, inclusively. See alſo § 312, § 318, § 334, and § 307.

‡ See Principles of Electricity, from § 248 to § 310, inclusively; and ſee particularly § 307.

In part XIX. of my Principles of Electricity *, I have enumerated "eleven neceſſary requiſites" in erecting conductors to ſecure buildings againſt damage by lightning. The ninth requiſite is †, "that there be neither large nor prominent bodies "of metal, upon the top of the building propoſed to be "ſecured, but ſuch as are connected with the conductor" (and conſequently with the common ſtock) "by ſome proper metal-"lic communication." And in ſection 538, I ſtate, that the "conſideration of the electrical returning ſtroke fully evinces the "utility of ſuch precaution." The circumſtances mentioned by Mr. BRYDONE, that "ſplinters had been thrown off in "many places, particularly where the timber of the cart was "connected by nails, or cramps of iron," ſtill more fully proves that ſuch precaution is right.

§ 32. The tenth ‡ neceſſary requiſite in erecting conductors, mentioned in my Principles of Electricity, is, "that there be "a ſufficient number of rods." And in ſection 542 I ſtate that, "the higheſt parts of a building, the moſt elevated "ridges, all the very prominent ſtacks of chimneys, and all "the moſt ſalient angles, ought, in order for the building to "acquire the greateſt degree of ſecurity, to be armed with "an high, tapering, and acutely pointed metallic conductor, "properly connected with the common ſtock. And upon edi-"fices of great importance (eſpecially magazines of gun-"powder) the pointed conductors ought never to be above

* See Principles of Electricity, from § 519 to § 544, inclusively; and particularly ſee § 519.

† See Principles of Electricity, § 519 and § 538.

‡ See Principles of Electricity, § 519; and from § 539 to § 542, inclusively.

"forty

" forty or fifty feet aſunder; and, if they were to be at ſtill " ſmaller diſtances aſunder, the ſecurity they would afford " would be ſtill more perfect."

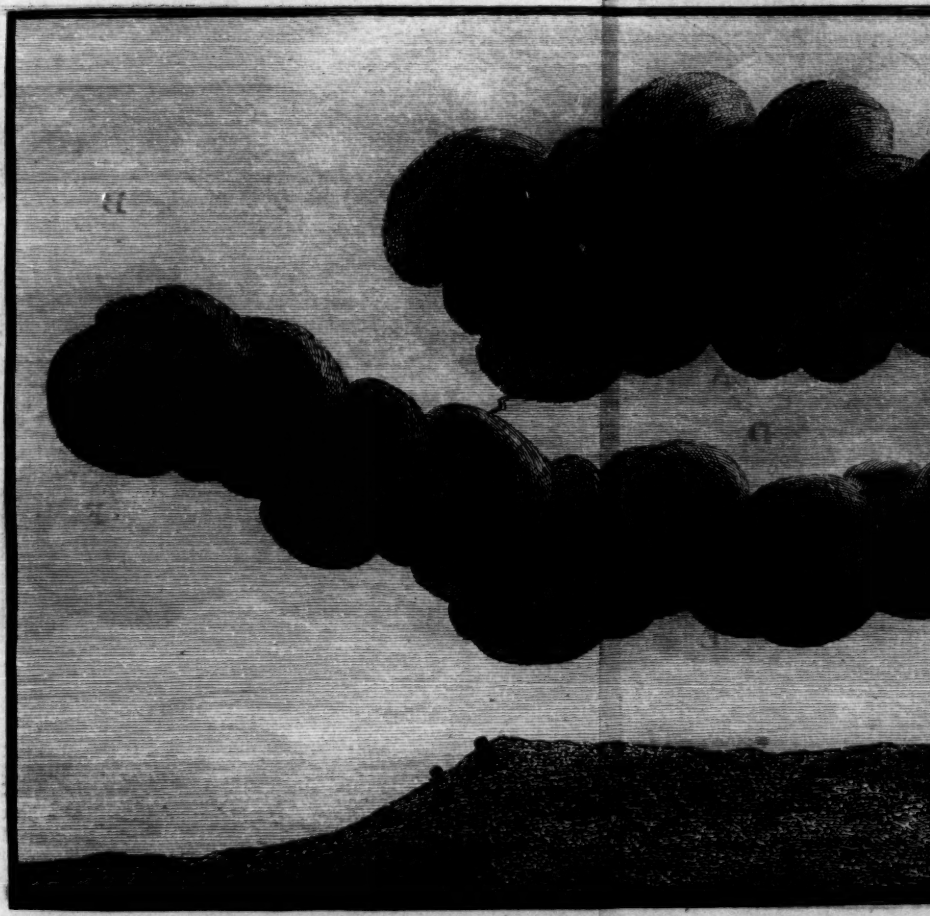
The reaſon of this precaution is fully explained in many parts of the above-mentioned Treatiſe, particularly in ſection 423*; and the circumſtance, very worthy of obſervation, related by Mr. BRYDONE, namely, that the diſtance was only " about *twenty-four yards*," between LAUDER, who was killed, and his companion, who " was ſenſible of no ſhock, nor un- " common ſenſation," does clearly demonſtrate the propriety of this precaution, of erecting *ſeveral* † conducting rods upon an extenſive building.

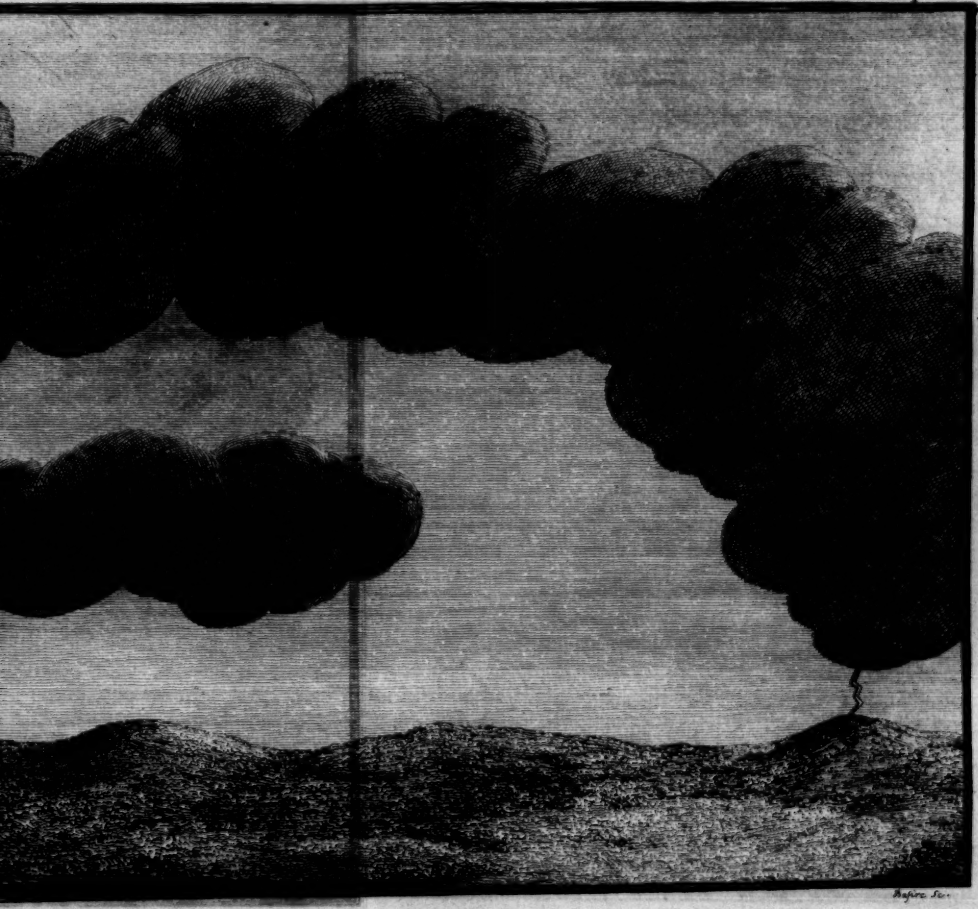
This information muſt be particularly intereſting to the Board of Ordnance, on account of the ſecurity of their magazines, particularly their powder magazines at Purfleet. The

* See alſo Part XVIII. of my Principles of Electricity (from § 494 to § 518 inclufively), where I have clearly demonſtrated this propoſition, namely, that, " *high and pointed metallic conductors* [when properly conſtructed, and when made to " communicate completely with the common ſtock] tend not only to prevent " a *main ſtroke of lightning* and the *lateral explofion*; but tend likewiſe moſt power- " fully to prevent any dangerous *electrical returning ſtroke* whatever from taking " place near that part of the edifice upon which they are erected. So admirable, " and ſo extenſive, is the principle upon which is founded this ſimple and moſt " incomparable invention!"

† See alſo (in the volume LXVIII. of the Philoſophical Tranſactions, part I. p. 313. et ſeq.) the " Report of the Committee, appointed by the Royal Society, 600 " to conſider of the moſt effectual Method of ſecuring the Powder Magazines at " Purfleet againſt the Effects of Lightning, in compliance with the Requeſt of the " Board of Ordnance;" in which Report, *ſeveral* high and acutely-pointed conductors, properly connected with the bottom of the wells, are propoſed to be erected; and other precautions, founded on a like principle, are recommended.

ſecurity of thoſe magazines is a great national object, not only on account of their importance in time of war; but, alſo on account of their vicinity to the City of London. For, from the immenſe quantity of gunpowder they contain (when full), their ſituation upon the river Thames, and their being within the diſtance of only a few miles of London, thoſe magazines being blown up would probably produce, in the Metropolis, a violent ſhock, like that of an earthquake.





Hafire Sc.

XVIII. *Concerning the Latitude and Longitude of the Royal Observatory at* Greenwich; *with Remarks on a Memorial of the late M.* Caſſini de Thury. *By the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read Feb. 22, 1787.

MEMOIRE
Sur la jonction de Douvres à Londres.

Par M. Cassini de Thury, Directeur de l'Obſervatoire Royal; de la Société Royale de Londres, &c.

IL eſt intéreſſant pour le progrès de l'aſtronomie que l'on connaiſſe exactement la différence de longitude et de latitude entre les deux plus fameux Obſervatoires de l'Europe; et quoique les obſervations aſtronomiques faites depuis un ſiècle offrent un moyen aſſez exact pour parvenir à cette recherche, il parait cependant que l'on n'eſt point d'accord ſur la longitude de Greenwich à onze ſecondes près, et ſur ſa latitude à quinze ſecondes.

L'on a reconnu par les opérations trigonométriques exécutées en France, au Nord, et au Perou, que ſur l'étendue d'un degré du méridien ou de 57 mille toiſes, l'on ſe trompait à peine de dix toiſes, ce qui a été prouvé par des baſes meſurées à l'extrémité des ſuites de triangles; ainſi ſur la diſtance de Douvres à Londres, qui eſt de 49800 toiſes ou environ, on ne pourrait ſe tromper de 120 toiſes, qui repondent à onze ſecondes en longitude.

M. CASSINI a déjà publié, dans le livre de la Méridienne Vérifiée, les opérations par leſquelles l'on a déterminé la diſtance de Calais à la groſſe tour de Douvres de 18241 toiſes par un premier triangle, et de 18243 toiſes par un ſecond triangle; on aurait cette diſtance avec une plus grande exactitude en obſervant les angles conclus à Douvres, qui ſont fort aigus. M. CASSINI a découvert des côtes de France pluſieurs objets ſur les côtes d'Angleterre, qui ſeront viſibles de la tour de Douvres; et ſur cette première baſe on établirait une ſuite de quelques triangles juſqu'à Londres, dont le nombre et la grandeur dépendent de l'expoſition des objets compris dans la direction de Douvres à Londres.

M. CASSINI ne doute point que ce projet ne ſoit agréé d'un Souverain qui aime les ſciences, qui non content des découvertes du celèbre COOK vient d'ordonner un ſecond voyage autour du monde, et que la Société Royale ne charge un de ſes Membres de l'exécution; et dans le cas où ſes occupations l'empêcheraient de s'y livrer, qu'elle ne permît à M. CASSINI de s'en charger. L'honneur qu'elle lui a fait de l'aſſocier à un corps auſſi reſpectable ſerait un titre pour lui accorder ſa confiance. M. CASSINI a profité du voyage du Roi en Flanders en 1748 pour joindre les triangles de la méridienne à ceux de SNELLIUS en Hollande; en 1762 il a prolongé la perpendiculaire de Paris juſqu'à Vienne en Autriche. La branche qui s'étendra juſqu'à Londres ſera la troiſième, et formera la jonction des deux plus belles villes de l'Europe.

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THE preceding Memorial of the late M. CASSINI DE THURY was put into my hands by Sir JOSEPH BANKS, our President, on the 28th of April, 1785, desiring me at the same time to give an answer to it. Happy if I can solve the doubts entertained by the late Royal Astronomer of France concerning the latitude and longitude of this Royal Observatory, and at the same time do justice to the memories of my learned predecessors, and to myself, I shall give an account of the principal operations that have been performed here for ascertaining those points, and then add my own remarks to elucidate the subject and reconcile the difficulties in question.

Had Dr. BRADLEY lived longer, for the benefit of astronomy, to publish his valuable observations, or had they been since published by another hand, which unfortunately they hitherto have not, these remarks might have been unnecessary, and perhaps even the occasion for them might never have occurred; as it would have then appeared upon what foundation the latitude of this Observatory had been established, and what differences of meridians between Greenwich and the other principal Observatories of Europe resulted from the observed eclipses of Jupiter's satellites and other celestial phænomena.

However, having formerly been apprised by Dr. BRADLEY himself of several particulars of moment relative to his observations, and particularly of the method which he used for settling his latitude and refractions, after he became possessed of the new instruments in 1750, and being assisted with some of his manuscript calculations, with the addition of my own observations, I flatter myself I can throw the light wanted on the

queſtion, and obviate the principal difficulty, that relative to the difference of latitude of Greenwich and Paris, and reduce the difference of meridians within ſmaller limits, notwithſtanding Dr. BRADLEY's original obſervations had been removed from this Obſervatory, in which they were made, before I came here, and have not yet been reſtored to it.

Dr. BRADLEY having been furniſhed by Government in the year 1750 with a braſs mural quadrant of eight feet radius, conſtructed by that excellent artiſt Mr. JOHN BIRD, an inſtrument far ſuperior to any before uſed in the practice of aſtronomy, aſſiduouſly obſerved the pole ſtar and other ſtars lying to the north of the zenith with it for upwards of three years, and then removed it to the oppoſite ſide of the wall, making it change place with the iron quadrant of the ſame radius conſtructed by Mr. GRAHAM, likewiſe an excellent inſtrument, though inferior to this, and commenced a regular ſeries of obſervations of the ſun, planets, and fixed ſtars, which have been ever ſince continued in the ſame manner. Moreover, the temperature of the air, ſhewn by the barometer and thermometer, is affixed to each obſervation; and the zenith point of the quadrant ſettled from time to time by the help of a zenith ſector of 12½ feet radius, turned alternately contrary ways, the ſame with which Dr. BRADLEY had before made his two uſeful and admirable diſcoveries of the aberration of light and the nutation of the earth's axis.

By the obſervations of the pole ſtar and other circumpolar ſtars, above and below the pole, Dr. BRADLEY got the apparent zenith diſtance of the pole; by the apparent and equal zenith diſtances of the ſun at the two equinoxes, having at the ſame time oppoſite right aſcenſions, as found from comparing his obſerved tranſits over the meridian with thoſe of

fixed ſtars, after the manner uſed by Mr. FLAMSTEED for deducing the right aſcenſions of the fixed ſtars, he found the apparent zenith diſtance of the equator, which leſſened by parallax and added to the apparent zenith diſtance of the pole gave a ſum leſs than 90° by the ſum of the two refractions belonging to the pole and meridian zenith diſtance of the equator. But he remarked, that the difference of refractions, belonging to theſe zenith diſtances, would come out the ſame within 2 or 3″ by any of the beſt tables then extant, whether deduced ſolely from obſervations, or partly from obſervations and partly from theory. The ſum and difference of refractions anſwering to the pole and equator being thus given, the refractions themſelves are given, the greater of which added to the apparent zenith diſtance of the equator gives the latitude of the place, and the leſs refraction added to the apparent zenith diſtance of the pole gives the co-latitude.

He afterwards, from the conſideration that the refractions at the pole and equator may be taken without ſenſible error as the tangents of the zenith diſtances, according to Mr. THOMAS SIMPSON's theory of refractions in his Mathematical Diſſertations, divided more accurately the ſum of the refractions at the pole and the equator into the juſt parts anſwering to each zenith diſtance, and thereby found the latitude with more exactneſs. In this manner he found the latitude of the Royal Obſervatory to be 51° 28′ 39″¼, and the mean refraction at 45° 3′ to be 57″, the barometer ſtanding at 29,6 inches, and the thermometer of FAHRENHEIT's ſcale at 50°.

But, not to let a matter of ſo much conſequence reſt on my aſſertion or memory, when further proof can be given of it, I have by me, in the hand-writing of Dr. BRADLEY, among other particulars, his calculations of the latitude of the Obſer-

vatory from his obſervations, according to the manner above explained; in which he firſt ſtates it at 51° 28′ 38″, and finally more correctly in theſe words. "The apparent zenith "diſtance of the equator, by the mean of 20 obſervations in "1746-47, 51° 27′ 28″. The mean apparent diſtance of the "pole, by the obſervations made between 1750-52, 38° 30′ 35″. "Sum 89° 58′ 3″. Sum of refractions 1′ 57″. Polar refrac-"tion 0′ 45″¼. Equatorial refraction 1′ 11″¼. Latitude "51° 28′ 39″¼. Co-latitude 38° 31′ 20″¼."

The latitude of the Obſervatory being thus ſettled, as well as the quantity of refractions for all ſtars paſſing the meridian between the pole and the equator, Dr. BRADLEY readily inferred from his obſervations the true diſtance of all ſuch ſtars from the north pole, which, compared with their zenith diſtances obſerved below the pole, gave the refractions at thoſe lower altitudes. Finally, by comparing the refractions together obſerved in extreme degrees of heat and cold, he deduced the law of their variation as affected by heat and cold; and thus at length he inferred his elegant rule for determining the refraction in all circumſtances, that it is to 57″, in the direct compound ratio of the tangent of the apparent zenith diſtance leſſened by 3 times the refraction to the radius, and of the height of the barometer in inches to 29,6 inches, and in the inverſe ratio of the degree of height of FAHRENHEIT's thermometer increaſed by 350 to 400.

But it may be proper to confirm this rule for refractions alſo from the ſame manuſcript of Dr. BRADLEY, which I before cited for confirming the latitude, by the following paſſage, which immediately follows the other. "Suppoſe the "mean refraction at 45° 3′ = 57″, and $y = 350$; then "$y+1$: bar. :: 77″ : refr. at 45° 3′.

"Rad.

" Rad. : tan. ZD :: 3′ : *m* . . . . Rad. : $\overline{\text{tan. ZD} - m}$ :: refr. at 45° 3′ : *r* " [the refraction required]." It is easy to see that this rule agrees with the other: for putting $t = 50$, and barometer $= 29{,}6$, the first analogy, putting the barometer down in tenths of an inch, is $350 + 50 = 400 : 296 :: 77'' : 56'',98$ for the refraction at 45° 3′, or 57″ within $\frac{1}{100}$th of a second. The second analogy serves to give the treble refraction nearly, called *m*. Whence it is evident, the last analogy coincides with the rule above given.

This valuable rule was first communicated by myself to the public in vol. LIV. of the Philosophical Transactions, p. 265. and in p. 49. and 129. of the first edition of Tables requisite to be used with the Nautical Almanac, together with a Table of the mean Refractions deduced from it, with the first Nautical Almanac, that of 1767, published by order of the Commissioners of Longitude in 1765; and again, at page the 5th of the Explanation and Use of the Astronomical Tables, annexed to the first volume of my Observations made at the Royal Observatory from 1765 to 1774, published by order of the President and Council of the Royal Society, with two tables in that work, containing the mean refractions and decimal multipliers for reducing them to any given temperature of the air indicated by the barometer and thermometer. The words in page the 5th of the said preface are as follows. " The " astronomical refractions and latitude of the Observatory were " settled with the greatest accuracy by Dr. BRADLEY, from his " observations of the circumpolar stars, with the brass mural " quadrant, during the three years that it was turned to the north, " and of the sun and stars in the subsequent years after it was " removed to point to the south. The following elegant rule " was

"was the result of his observations, that the refraction at any "altitude is to 57 seconds, in the direct compound ratio of the "tangent of the apparent zenith distance lessened by 3 times "the refraction to the radius, and of the altitude of the baro- "meter in inches to 29,6 inches, and in the reciprocal ratio "of the height of FAHRENHEIT's thermometer increased by "the number 350, to the number 400. Tables XXII. and "XXIII. were adapted to this rule; the first containing the "mean refractions answering to 29,6 inches height of the "barometer and 50 degrees height of the thermometer; and "the second table containing decimals for multiplying the "mean refraction in order to find the correction, which applied "to it will give the actual refraction, the same as would have "been produced by the rule with somewhat more trouble. "Dr. BRADLEY supposed the horizontal parallax of the sun "10½ seconds, in the calculations from which he inferred the "refractions; and I have been informed, that he determined "the latitude of the Observatory 51° 28′ 39″½. But, had he "made use of the true parallax 8″8 or 8″½, as found by the "two late transits of Venus over the sun, he would have made "the refraction at the altitude of 45° to be 56″½ instead of "57″, and the latitude of the Observatory exactly 51° 28′ 40″ "instead of 51° 28′ 39″½. But his rule for refractions can- "not be corrected for all altitudes, without examining his ob- "servations of refractions made at various times."

On comparing this extract with M. CASSINI's Memoir, I cannot but express my surprise, that he should not have adverted to a passage containing so direct an application to the grounds of his Memorial, in a publication of such notoriety, and of so old a date as 1776; had he done so, I cannot but think, he would never have hazarded such an opinion as that advanced by him

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in his Memoir of an uncertainty of 15″ in the latitude of Greenwich; but he might have been induced to believe, that the latitude of this place had been well determined.

For further confirmation of the certainty of the aſtronomical refractions, and latitude of the Obſervatory, as ſettled by Dr. BRADLEY, it may be proper to add, that the Greenwich braſs mural quadrant underwent a trial, which all aſtronomical inſtruments ought to be ſubmitted to, but which very few ever have been, on account of the difficulty and nicety of the operation, namely, an examination of the total arc; when it was found by Dr. BRADLEY to be an accurate quadrant, the arc appearing at one trial to differ only a fraction of a ſecond from 90°, and another time, after an interval of above ſix years, to be a perfect quadrant. See p. 24. of BIRD's Method of conſtructing Mural Quadrants, publiſhed by the Board of Longitude in 1768. In like manner he had before examined the total arc of the iron quadrant, firſt put up by Mr. GRAHAM, for the uſe of Dr. HALLEY, in the year 1725, by means of a level, and found it to be 16″ leſs than a quadrant. See BIRD's Method of conſtructing Mural Quadrants, p. 7. and Memoires of the Royal Academy of Sciences at Paris, for 1752, p. 424. But this quadrant was, in the year 1753, re-divided by Mr. BIRD, and, in this reſpect, probably rendered as accurate as the other. See BIRD's Method of conſtructing Mural Quadrants, p. 24.

Dr. BRADLEY made a curious uſe of the new ſet of diviſions, ſoon after they were laid upon the quadrant, to re-examine the error of the total arc laid down originally by Mr. GRAHAM (which by the plumb-line and level he had found to be 16″ leſs than a quadrant in 1745) according to the following paſſage contained in the manuſcript before cited.

"Auguſt

"August 12, 1753, I measured with the screw of my micrometer the difference of the arcs (of $\frac{60}{77}$) as set off by "Mr. GRAHAM originally, and by Mr. BIRD when he put "on a new set of divisions upon the old quadrant, and I "found that Mr. GRAHAM's arc was less than Mr. BIRD's by "$\frac{9}{10}$ divisions of my micrometer, which to a radius of 96 inches "answers to 10″,6; so that the whole arc of 96 differs from "a true quadrant 15″,9, which is the same difference that I "formerly found by means of the level, &c."

Let me further add, that Dr. BRADLEY had informed me, that he had found the same refractions, latitude of the Observatory, and obliquity of the ecliptic, by both quadrants, making a proportionable allowance, in the use of the iron quadrant, for the error of 16″ in the total arc in proportion to the zenith distance of the object before it was new divided.

The Rev. Dr. HORNSBY, F. R. S. Savilian Professor of Astronomy at Oxford, to whose care Dr. BRADLEY's original observations have been committed, in order to their being printed and published, having favoured me with calculations of the latitude of the Royal Observatory from observations of the pole star made with both quadrants, from a manuscript of Dr. BRADLEY, I think it proper to give it a place here, not only as a very curious paper, but also as strongly confirming the latitude of this place before stated.

Transcribed from a loose Paper of Dr. BRADLEY.

"The mean zenith distances of the pole star above and below "the pole, corrected by refraction, aberration, &c. and re-"duced to January 1751, O. St. as collected from the obser-"vations made after the new quadrant was balanced, Nov. "24, 1750.

Number

Number of observations.	Above the pole.	Number of observations.	Below the pole.	to
23	36 29 40,66	8	40 33 2,95	Jan. 15, 1751
22	45,42	26	3,37	Aug. 27, 1751
23	45,13	27	2,14	May 4, 1752
19	44,63	28	1,67	Nov. 10, 1752
28	45,00	23	1,74	May 31, 1753
9	44,59	8	1,54	July 26, 1753
124	36 29 45,24	120	40 33 2,24	
			36 29 45,24	
			77 2 47,48	
			38 31 23,74	
		Error of collimation	1,74	
		Co-latitude	38 31 22, 0	

77 1 15,8

		Refraction.			
	38 30 37,9				4 3 10,8
Collimation	1,4	42″ +	36 29 2½		
Refraction	+ 45,5	49 −	40 32 13½		+ 6,6
	38 31 22,0	6½	4 3 11		4 3 17,4
By new quad.	51 28 38,0	Refraction	+ 6½		
			4 3 17½		
			2 1 39 − the mean distance from mean pole Jan. 1751.		

The apparent zenith distance of the pole, by the mean of 310 observations, is

38° 30′ 36″ allowing − 2″ for the error of the line of collimation.

Refraction + 45½

38 31 21½ Co-lat. 38° 31′ 21″½ by the new quadrant.

Latitude 51 28 38½ 38 31 18½ by the old quad. new divisions.

Apparent zenith distances of the pole observed with the iron quadrant.

1753	Apparent zenith distance of the pole.	Barom.	Thermom. in.	Thermom. out.
	° ′ ″	Inches.	°	°
Sept. 13	38 30 40,1	30,10	64	65
23	41,5	29,88	61	60
Oct. 2	42,5	29,67	56	53
5	41,0	30,02	57	57
11	41,1	29,48	61	57
19	40,2	29,82	50	43
31	40,8	29,65	42	34
Nov. 5	42,1	29,69	45	39
16	40,5	29,39	42	35
19	42,2	29,59	40	35
20	40,9	29,84	40	33
24	40,0	30,00	43	35
29	40,0	29,80	38	30
Dec. 3	39,3	30,16	39	33
8	37,4	30,06	35	27
17	41,5	29,50	50	49
30	38,0	30,11	33	22
Mean	38 30 40,5	29,81	47	41½
Refraction	+ 46,4			
Col.	− 8,4			
Pol. corr.	38 31 18,5			

So far the manuscript.

Thus the latitude by the brass quadrant being 51° 28′ 38″½, and by the iron quadrant with new divisions 51° 28′ 41″½, the mean by both quadrants is 51° 28′ 40″, or only half a second greater than settled in another manner, according to the manuscript of Dr. BRADLEY in my possession. Also the apparent zenith distance of the pole with the mean refraction 45″,4 being 38° 30′ 36″,1 by the brass quadrant, and 38° 30′ 33″,1, by the iron quadrant, the mean by both is

38°

38° 30′ 34″,6, or only $\frac{4}{10}$ths of a second less than by Dr. BRADLEY's manuscript cited before.

On my promotion to the Royal Observatory in 1765, finding its latitude to have been so accurately settled by Dr. BRADLEY before me, I might have thought myself dispensed from making any particular or very laborious observations for that purpose; however, I confirmed it by my own observations to great nearness, *viz.* within 1 or 2″, at the same time that I was establishing a new catalogue of the principal fixed stars, continually observed here for settling the right ascensions of all other celestial objects with the transit instrument. The result in brief is as follows.

I first settled the relative right ascensions of about 30 of the brightest fixed stars, and lying nearest the equator, by a great number of observations with the transit instrument, referring them to α Aquilæ as the fundamental star, whose right ascension I assumed from Dr. BRADLEY's determination. Hence, by observed transits of the sun and the same stars, in the spring and autumn, when his daily motion in declination was at least 16′ or two-thirds of the greatest, I inferred the sun's right ascensions relative to the right ascensions of those stars settled in the manner just mentioned. Also from the sun's observed zenith distances taken with the brass mural quadrant on the same days, and corrected by refraction, parallax, and error of line of collimation, with Dr. BRADLEY's obliquity of the ecliptic, and latitude of the Observatory, I computed the sun's declinations, and thence the right ascensions corresponding to them.

Now, if the assumed right ascension of α Aquilæ, and thence those of the other stars were affected with some small error, as might be supposed, the sun's right ascensions deduced from the observed transits would differ the same way from the

truth at both seasons of the year, *viz.* by the unknown error of the assumed right ascension of *α* Aquilæ; but his right ascensions inferred from his observed zenith distances would be affected contrary ways at the two opposite seasons of the year, by the unknown errors in the refractions, parallaxes, latitude of the place, and obliquity of the ecliptic. Hence, the mean of the two corrections of the sun's right ascension, found from the observed declinations about the vernal and autumnal equinox, would be the true correction of the assumed right ascension of *α* Aquilæ; and the difference of the same corrections would, by an easy calculation, shew how much the computed declinations were too great or too little for the truth, and consequently what the true declinations were, and what the true zenith distance of the sun was, when in the equator, or the latitude of the place, on supposition that Dr. BRADLEY's refractions were truly stated; for any small uncertainty in the obliquity of the ecliptic, as stated by him, could not affect this result, which was deduced equally from observations of the sun in north and south declination, when the same error of the obliquity would affect the sun's right ascensions deduced from the observed declinations contrary ways. I took the sun's parallax from the 24th of my tables annexed to my observations, constructed upon a horizontal parallax 8″,84, which I had deduced from the observations of the first transit of Venus, that in 1761, and differing insensibly from 8″¼, which I deduced from the observations of the total durations of the transit between the internal contacts observed at Wardhus and Otaheite in 1769, consequently more correct than the horizontal parallax of 10″¼ used by Dr. BRADLEY. It is also evident, that the true zenith distance of the equator thus found, diminished by Dr. BRADLEY's mean refraction, will be the apparent

zenith distance of the equator, affected only by the mean refraction.

I shall now give the apparent zenith distance of the equator, and the true latitude of the Observatory, resulting in this manner from my observations of six years, from the autumnal equinox of 1765 to that of 1771, in which I allowed 0″,9 for the correction of the error of the line of collimation, additive to the observed zenith distances, as I found from a revision of my calculations of the zenith distances of stars taken with the mural quadrant, compared with the like taken with the zenith sector in 1768.

Years of the observations.	Number of days of observations.	Apparent zenith distance of equator, taking the sun's horizontal parallax 8″,8.	True latitude of the Observatory, according to Dr. BRADLEY's refractions and the sun's horizontal parallax 8″,8.
Autumnal equinox of 1765 and vernal of 1766	52	51° 27′ 28″,6	51° 28′ 40″,1
Autumnal equinox of 1766 and vernal of 1767	38	30,4	41,9
Both equinoxes of 1768 – –	46	32,2	43,7
Both equinoxes of 1769 – –	48	28,7	40,3
Both equinoxes of 1770 – –	20	29,3	40,8
Both equinoxes of 1771 – –	42	29,7	41,3
Mean from six years observations – –		51 27 29,8	51 28 41,3
Mean found by Dr. BRADLEY, with ☉'s horizontal parallax 10″½ – –		51 27 28	51 28 39,5
But if 1″,2 be added to reduce them to the ☉'s horizontal parallax 8″,8, Dr. BRADLEY's result will be changed to –		51 27 29,2	51 28 40,7
Differing from my determination above only		0,6	0,6

In further confirmation of the latitude of the Observatory, I shall now adduce eight years observed zenith distances of the sun in the solstices, being deduced from a number of observations taken at and near the solstices, and corrected for line of collimation, refraction, parallax, and nutation.

Years.

Years of obſervations.	Summer ſolſtitial zenith diſtance reduced.	N° of days of obſervations.	Winter ſolſtitial zenith diſtance.	N° of days of obſervations.	Half ſum or latitude of the place.	Half difference or obliquity of the ecliptic.
	° ′ ″		° ′ ″		° ′ ″	° ′ ″
1765	28 0 30,2	6	74 56 46,1	5	51 28 38,2	23 28 8,0
1766	33,6	7	48,8	2	41,2	7,6
1767	32,0	6	uncertain from an accident.	—	—	—
1768	29,7	6	44,9	7	37,3	7,6
1769	31,8	8	46,2	5	39,0	7,2
1770	30,7	7	44,1	6	37,4	6,7
1771	32,0	8	42,2	9	37,1	5,1
1772	31,8	12	Line of collimation altered by applying an achromatic object-glaſs to the teleſcope.			
Mean	28 0 31,5		74 56 45,4		51 28 38,4	23 28 7,0
Latitude	51 28 40					On Jan. 1, 1769.
	23 28 8,5 mean obliquity of ecliptic on Jan. 1, 1769.					

This laſt obliquity 23° 28′ 8″,5 is deduced in the manner uſed by Dr. BRADLEY, and is more to be depended on than the other 23° 28′ 7″,0 deduced from both ſolſtices, on account of the leſs certainty of the lower refractions, from which, however, it only differs a ſecond and a half.

Thus all the obſervations of the ſun and circumpolar ſtars accord to 1″ or 2″ with the latitude of the Obſervatory ſettled by Dr. BRADLEY, making uſe of his refractions.

I ſhall now determine the latitude independent of Dr. BRADLEY's refractions, and infer the higher refractions at the ſame time, from a compariſon of my obſervations of the apparent zenith diſtance of the equator before ſet down with Dr. BRADLEY's obſervations of the apparent zenith diſtance of the pole, both taken with the ſame excellent braſs mural quadrant, in the ſame manner as Dr. BRADLEY deduced them from

from the apparent zenith distance of the equator observed with the iron quadrant compared with the apparent zenith distance of the pole observed with the brass quadrant, according to the extract from the manuscript in my possession before cited.

The mean apparent zenith distance of the equator, by my observations of six years from 1765 to 1771, was related before 51° 27′ 29″,8. The mean apparent zenith distance of the pole was found by Dr. BRADLEY from 1750 to 1752 to be 38° 30′ 35″. Their sum 89° 58′ 4″,8 taken from 90° leaves 1′ 55″,2, the sum of the refractions at the two zenith distances. Saying then, as 1′ 56″,7 the sum of the refractions by Dr. BRADLEY's rule, to 1′ 55″,2 the sum by observation, so are 1′ 11″,4 and 45″,3 the respective refractions at the two apparent zenith distances of the equator and pole by Dr. BRADLEY's rule, to 1′ 10″,5 and 44″,7 the two refractions at those zenith distances, which added to them give the co-latitude 38° 31′ 19″,7, and the latitude 51° 28′ 40″,3. And as 1′ 56″,7 : 1′ 55″,8 :: so is 57″ the refraction at the apparent zenith distance 45° 3′ by Dr. BRADLEY : 56″,27 the true refraction at that zenith distance, or not half a second differing from Dr. BRADLEY's, but more to be depended on as deduced from observations made with the brass quadrant only, and calculated from a parallax of the sun nearer to the truth.

But if the apparent zenith distance of the pole be made use of, resulting from a mean of 310 observations made with the brass quadrant, according to Dr. BRADLEY's manuscript, communicated by Dr. HORNSBY, from the whole of his observations from 1750 to 1753, *viz.* 38° 30′ 36″, the sum of this and 51° 27′ 29″,8, the apparent zenith distance of the equator found by myself with the same instrument, or 89° 58′ 5″,8 taken from 90° leaves 1′ 54″,2, the sum of the two refractions

at

at the pole and equator. Whence the refraction at the pole will be found in like manner as before 44″,3, and that at the equator 1′ 9″,9, and the latitude 51° 28′ 39″,7, and the refraction at the apparent zenith diſtance of 45° 3′ = 55″,8, which is 1″, 2 leſs than Dr. BRADLEY's determination, and 1″,2 greater than deduced from Mr. HAWKSBEE's experiment of the refraction of the air hereafter cited. It will be ſhewn in the ſequel, that the latitude thus found does not at all depend on the truth of the total arc, but only ſuppoſes the inſtrument proportionally divided at the points anſwering to the pole and the equator.

From the whole then I conclude, that the latitude of the Royal Obſervatory at Greenwich is firmly eſtabliſhed from Dr. BRADLEY's obſervations and my own at 51° 28′ 40″, probably without the error of a ſingle ſecond.

Let us now inquire into the latitude of the Royal Obſervatory at Paris. M. LE MONNIER, in the Memoires of the Royal Academy of Sciences for 1738, and in his *Hiſtoire Celeſte*, has examined into the latitude of the Royal Obſervatory at Paris, reſulting from the obſervations of the principal French aſtronomers, and aſſuming the refraction at the height of the pole at Paris to be 50″, which is 2″ leſs than DOMINICO CASSINI's table gives, and the ſame which Dr. BRADLEY's rule gives, he finds the latitude of their Royal Obſervatory as follows:

From the obſervations of M. PICARD -	48°	50′	10″
——————— of M. DE LA HIRE -	48	50	12
——————— le Chev. DE LOUVILLE	48	50	8
——————— M. Maraldi -	48	50	14

His

	°	′	″
His own observations in 1738, after examining and making an allowance for the error of the total arc of his quadrant - - - -	48	50	14
His further observations in 1740, making allowance for the error of the total arc of his quadrant, and considering the effect of the state of the air indicated by the thermometer upon the refractions	48	50	15
In the Memoires of the Royal Academy of Sciences for 1744, M. Cassini de Thury (the author of the memoir) finds from his own observations, with the same refractions - - - -	48	50	12
In the Memoires of 1755, the Abbé de la Caille, from a nice and accurate calculation of his observations made at the College of Mazarine, at Paris, and the Cape of Good Hope, deduces new tables of refraction suitable to each place, and states their respective latitudes, and thence that of the Royal Observatory at Paris - - -	48	50	14
Hence the ancient observations of M. Picard, M. de la Hire, and the Chevalier de Louville give	48	50	10
The modern and more accurate observations of M. Maraldi, M. le Monnier, M. Cassini de Thury, and the Abbé de la Caille, give -	48	50	14;

which is now generally made use of by the French astronomers as the true latitude of their Royal Observatory; and from the near agreement of so many diligent observers and able astronomers cannot be supposed to differ above 2 or 3″ from the truth. The difference of this and 51° 28′ 40″, the latitude of the Royal Observatory at Greenwich above stated, is 2° 38′ 26″, the true difference of latitude of the two Observatories, which,

from what has been ſaid of the obſervations on which the reſpective latitudes were founded, cannot be ſuppoſed to differ above 3 or 4″ from the truth. What then becomes of the uncertainty of 15″ ſuppoſed by the late M. CASSINI?

The ſame difference of latitude I find nearly from a compariſon of my own obſervations of γ and β Draconis, taken with the zenith ſector in 1768, with thoſe of the Abbé DE LA CAILLE in 1750 and 1756, given in his *Fundamenta Aſtronomiæ*, after making the proper allowances for aberration, preceſſion, and nutation, and correcting my obſervations by Dr. BRADLEY's refraction, and the Abbé DE LA CAILLE's by his table, and making allowance for the diſtance of the Abbé DE LA CAILLE's Obſervatory from their Royal Obſervatory; *viz.* 2° 38′ 25″,4 from γ Draconis, and 2° 38′ 26″,1 from β Draconis; the mean being 2° 38′ 25″,7, differing only 0″,3 from that ſtated above; but from Dr. BRADLEY's obſervations 2° 38′ 24″9, and 2° 38′ 27″,2, mean 2° 38′ 26″,0. It is too well known to aſtronomers to need my pointing out, that the beſt method of determining the difference of latitude of places, differing but little in latitude, is by ſuch differences of zenith diſtances of ſtars paſſing near the zeniths, as the two above cited, obſerved at both places, in the ſame manner as the amplitude of the celeſtial arc is obſerved for finding the length of a degree of the meridian by compariſon with geometrical meaſures.

The queſtion now will be, upon what foundation was the late M. CASSINI's ſuppoſition of an uncertainty of 15″ in the latitude of Greenwich built? This appears evidently to have been upon a paſſage in the Abbé DE LA CAILLE's reſearches into the aſtronomical refractions and latitude of Paris, contained in the Memoires of the Royal Academy of Sciences for

1755,

1755, p. 578, 579, where M. DE LA CAILLE takes the differences of zenith distances of 14 stars observed by Dr. BRADLEY (in correspondence to the same observed by himself at the Cape of Good Hope, for determining the moon's parallax in declination) published in the Memoires of the Royal Academy of Sciences for 1752, and the same observed by himself at Paris, after his return from the Cape, and correcting them for the difference of the refractions at the respective zenith distances, according to his own table of refractions, and the known apparent motions of the stars, finds the mean 2° 37′ 23″,9, which added to 48° 51′ 29″,3, his latitude at the College of Mazarine, gave him 51° 28′ 53″,2 for the latitude of Greenwich, exceeding Dr. BRADLEY's latitude by 13 or 14″.

Now the legitimacy of this conclusion depends upon a supposition that both instruments measured the true angle, or that their total arcs were justly laid off, and that the Abbé DE LA CAILLE's table of refractions is just. The first indeed has been proved with respect to Dr. BRADLEY's quadrant, but never has been attempted with respect to the Abbé DE LA CAILLE's sextant; for the examination which the Abbé made of his instrument by parts for every 7°½ (see Memoires of the Royal Academy of Sciences for 1751 p. 405.), could not determine the error of the whole arc, as the difference from the truth might be insensible upon such small arcs, and the examination seems to have been intended to find the differences of these small arcs from one another rather than from the true arc which they represent. We may therefore be allowed to doubt of the truth of this circumstance. This doubt will be further strengthened by several particulars which I shall adduce.

1. The apparent altitude of the pole at the Royal Observatory 48° 51′ 12″, resulting from the Abbé DE LA CAILLE's

obſervations, exceeds 48° 51′ 4″ the mean of the obſervations of Meſſ. MARALDI, LE MONNIER, and CASSINI DE THURY, by 8″, and at the ſame time his refractions for that altitude exceed what they adopt by the ſame quantity. 2. His refractions are greater than all other tables give, DOMINICO CASSINI's, FLAMSTEED's, NEWTON's, BRADLEY's, MAYER's, SIMPSON's, and Lord MACCLESFIELD's. The latter I have by me in a manuſcript of Dr. BRADLEY, being what he uſed to correct his obſervations by, before he had been enabled to determine the refractions with the new mural arc. They were deduced from a braſs quadrant of 5-feet radius made by Mr. SISSON, ſtill remaining in the Obſervatory at Sherburn-Caſtle, and are the more to be eſteemed becauſe the diviſions of the inſtrument had been ſubmitted to the *ſtricteſt re-examination*, whereby, in the opinion of Dr. BRADLEY, it was probably rendered *as perfect in its kind as any extant, or as human ſkill could at that time produce.* See Dr. BRADLEY's Letter to Lord MACCLESFIELD, Phil. Tranſ. vol. XLV. p. 5. The refractions in this table are leſs than Dr. BRADLEY's by 2″,4 at the altitude of 45°, and 4″ at the altitude of 20°. MAYER's refractions agree almoſt exactly with Dr. BRADLEY's, and are entitled to much weight, having been determined by a 6-feet mural arc conſtructed by Mr. BIRD. 3. The refractions were found by the French Academicians at the polar circle, according to M. MAUPERTUIS's Book on the Figure of the Earth, to agree nearly with DOMINICO CASSINI's table. Hence it may be inferred, that the refractions in a warmer climate, as France, ſhould be leſs than according to the ſame table, and therefore much leſs than according to M. DE LA CAILLE's, and approaching to Dr. BRADLEY's, which are a little leſs than M. CASSINI's. 4. M. LE MONNIER, after his return from the polar circle, with a quadrant examined at the zenith and horizon, and after making allowance for the error thereby inferred in the total arc, ob-

ſerved a great many refractions of ſtars under the pole, with the ſtate of the thermometer, and ſometimes of the barometer alſo, as recorded in his *Hiſtoire Céleſte*. Theſe I calculated formerly, and found the refractions obſerved in very hot and very cold weather, compared together, to follow the ſame rate of increaſe and decreaſe, according to the changes of temperature, as Dr. BRADLEY has aſſigned; and, reducing the obſerved refractions to the mean temperature, I found them agree nearly with Dr. BRADLEY's. 5. The refractive power of the air about its mean temperature was carefully obſerved by Mr. HAWKSBEE, as related in his Phyſico-Mechanical Experiments, and the ratio of the ſine of incidence to that of refraction out of air into a vacuum found to be as 999736 to 1000000. Hence the aſtronomical refraction at the altitude of 45° ſhould be 54″,6, only 2″,4 leſs than Dr. BRADLEY's, and 2″ leſs than the ſame when his higher refractions are new calculated with the true parallax of the ſun, and 1″,2 leſs than I have before ſhewn to reſult from my obſervations of the apparent zenith diſtance of the equator compared with Dr. BRADLEY's of the apparent zenith diſtance of the pole, both taken with the ſame braſs mural quadrant, but 12″ leſs than the Abbé DE LA CAILLE's.

From all theſe facts, I think, I may be allowed to conclude, that the Abbé DE LA CAILLE's refractions are not juſt, but conſiderably too large; and, conſequently, as there can be no doubt of the care or diligence uſed by this aſtronomer in his obſervations and calculations, that the total arc of his inſtrument is too large for the radius, and, as I ſhall ſhew preſently, gives the meaſures of the zenith diſtances too ſmall.

But it may aſked, are then all the obſervations of this great aſtronomer, with their reſults, the fruit of ſo much labour and pains, to be conſidered as uncertain or lowered in their value in proportion to the error of his inſtrument? I am happy to anſwer,

anſwer, that the very ingenious method which he uſed of getting his refractions from the compariſon of the ſum of the apparent altitudes of the poles at Paris and the Cape with the ſum of the apparent zenith diſtances of ſtars paſſing the meridian between the two places, has fortunately, without his being aware of it, given him the refractions affected with the error of the arc of the inſtrument, and conſequently proper for correcting his obſervations; for, if the inſtrument be ſuppoſed ill divided, any error in the diviſions will naturally be thrown upon the refractions; and, if the total arc is too large for the radius, the ſtars will appear to approach the zenith by the error of the diviſions as well as the refractions, and the refractions in the table will come out too large, but ſtill ſuitable to the inſtrument, becauſe a correction is neceſſary to be added to the obſerved zenith diſtance, on account of the error of the inſtrument, as well as of the true refractions, and the table deduced from the inſtrument gives the ſum of the two corrections together, without determining them ſeparately.

Hence his table of refractions, though well adapted to his inſtrument, may be very unfit to be applied to any other. His latitudes of his obſervatories and his declinations of the ſtars will not loſe any of their certainty, at leaſt within the limits of the zenith diſtances meaſured by his ſector, *viz.* 60°. And this accounts for a circumſtance, at firſt ſight rather extraordinary, that his declinations of ſtars ſhould agree ſo nearly (generally within 5″ of Dr. BRADLEY's, as Dr. BRADLEY himſelf remarked) though his refractions made uſe of were ſo very different.

Having now ſhewn, that the Abbé DE LA CAILLE's refractions are too great, and only fit to be applied to his own inſtrument, it will be eaſy, by a juſt calculation, to reconcile the

before

before mentioned zenith distances of 14 stars, observed at Greenwich by Dr. BRADLEY and by the Abbé DE LA CAILLE at the College of Mazarine at Paris, with the established latitudes of the two Observatories, nearly; in doing which I shall claim the same right to correct Dr. BRADLEY's observations by his table of refractions, as I have allowed the Abbé DE LA CAILLE to be intitled to correct his observations by his table of refractions; which, I think, will be allowed me, after what I have said of the manner in which the Greenwich refractions were deduced and the instruments made use of. The difference of latitude of the College of Mazarine and the Royal Observatory at Greenwich will then come out by the several stars, as follows; 2° 37′ 12″,7, 16″,0, 13″,8, 13″,7, 18″,4, 17″,7, 19″,7, 23″,2, 17″,7, 17″,0, 13″,9, 12″,4, 5″,6, 11″,9. The mean is 2° 37′ 15″,2 (or 8″,7 less than the Abbé DE LA CAILLE's result in his method of calculation, which I have shewn to be inadmissible) and added to 48° 51′ 29″,3, the latitude of the Abbé DE LA CAILLE's Observatory gives 51° 28′ 44″,5 for the latitude of the Royal Observatory at Greenwich, only 4″½ more than established by Dr. BRADLEY's observations and my own; a sufficient agreement, especially considering that many of the stars were at great distances from the zenith, and that no account has been made of the temperature of the air at the times of the observations. The proper method, however, of settling the difference of latitude of two Observatories is by stars near the zenith, as I observed before; and the difference of the latitudes of the two Observatories of the College of Mazarine and Greenwich, by the Abbé DE LA CAILLE's observations of β and γ Draconis compared with mine, was 2° 37′ 10″,4, and compared with Dr. BRADLEY's 2° 37′ 10″,7, the first of which, added to the Abbé DE LA CAILLE's latitude, gives

51°

51° 28′ 39″,7, and the other 51° 28′ 40″, for the latitude of the Royal Obſervatory at Greenwich, exactly agreeing with that deduced immediately from the obſervations made at this place.

The ſame reſult nearly follows from M. CASSINI DE THURY's own obſervations of the zenith diſtance of the ſun at the ſummer ſolſtice of 1755, contained in the Memoires of the Royal Academy of Sciences for that year, compared with Dr. BRADLEY's, which latter was communicated to me by the late JOHN HOWE, Eſq.; for, by M. CASSINI's obſervations, the ſolſtitial altitude of the ſun's upper limb, corrected by the difference of refraction and parallax, according to DOMINICO CASSINI's table, which happens to agree with the ſame difference by my tables at this height, was 64° 53′ 36″, from which 15′ 47″ being ſubtracted for the ſemi-diameter of the ſun according to MAYER's tables, there remains 64° 37′ 49″, the true altitude of the ſun's center, and conſequently the ſun's true zenith diſtance 25° 22′ 11″. But the ſame was found from Dr. BRADLEY's obſervations, by my tables of refractions and the ſun's parallax, 28° 0′ 32″,8. The difference 2° 38′ 21″,8 or 2° 38′ 22″ is the difference of latitude of the two Obſervatories, which added to 48° 50′ 14″, the latitude of the Royal Obſervatory at Paris, gives 51° 28′ 36″ for the latitude of the Royal Obſervatory at Greenwich, or only 4″ leſs than before ſtated from the Greenwich obſervations, the difference lying the contrary way to that which the Abbé DE LA CAILLE carried the latitude of Greenwich, by improperly applying his own table of refractions to the Greenwich obſervations as well as to his own.

The Abbé DE LA CAILLE having, in the ſequel of his memoir, inferred the difference of latitude of Gottingen and the College of Mazarine from 22 ſtars obſerved by M. MAYER with

with a 6-feet mural quadrant of BIRD's conſtruction, correſpondent to the ſame obſerved by himſelf, I ſhall make ſome remarks on this compariſon, becauſe it appears to me to afford a ſtrong argument to ſhew that the Abbé DE LA CAILLE's refractions are too great; and that MAYER's, which agree with Dr. BRADLEY's, are juſt. The Abbé, after correcting the zenith diſtances of 22 ſtars obſerved at both places by his own table of refractions, finds the difference of latitude of Gottingen and Paris, by a mean, to be 2° 40′ 35″,1, which added to 48° 51′ 29″,3, the latitude of the College of Mazarine, gives him the latitude of M. MAYER's Obſervatory 51° 32′ 4″,4. He adds, that ſome obſervations of the pole ſtar ſent to him by M. MAYER would give the latitude of Gottingen 19″ leſs than he has eſtabliſhed it, as juſt mentioned. Now I find, that if M. MAYER's obſervations of the pole ſtar, as well as of the ſtars to the ſouth of the zenith, be corrected by M. MAYER's table of refractions, and the Abbé DE LA CAILLE's obſervations by *his* table of refractions, the latitude reſulting from M. MAYER's obſervations of the pole ſtar will agree to 2″ with that reſulting from the difference of latitude by the ſtars to the ſouth; for ſubtracting 19″ from 51° 32′ 4″,4, the latitude which the Abbé DE LA CAILLE has aſſigned to Gottingen in the manner above-mentioned, there remains 51° 31′ 45″,4 the latitude which he found by the pole ſtar; to which adding 52″,8, the refraction at the mean height of the pole ſtar according to the Abbé DE LA CAILLE, the ſum 51° 32′ 38″,2 muſt be the apparent height of the pole by Mr. MAYER's obſervations, which diminiſhed by 45″,6, M. MAYER's refraction, gives the true latitude of M. MAYER's Obſervatory 51° 31′ 52″,6. But by the difference of the Abbé DE LA CAILLE's and M. MAYER's zenith diſtances of the 22 ſtars to

the south, corrected each by their own table of refractions, I find the difference of latitude 2° 40′ 32″,0, 28″,9, 25″,1, 27″,9, 22″,6, 28″,3, 32″,0, 26″,7, 27″,2, 24″,2, 25″,4, 31″,3, 23″,2, 23″,6, 28″,9, 18″,4, 16″,7, 22″,0, 23″,8, 21″,1, 27″,3, 27″,3, the mean of which is 2° 40′ 25″,6, which, added to 48° 51′ 29″,3, the latitude of the College of Mazarine, gives the latitude of Gottingen 51° 31′ 54″,9, or only 2″,3 more than I have deduced above from M. MAYER's observations of the pole star rightly corrected, and only 0″,9 less than is set down in M. MAYER's tables, which he expressly says, p. 48. of the precepts to his solar and lunar tables, published by myself for the Commissioners of Longitude in 1770, was deduced from his own observations.

I have before, when I shewed the Abbé DE LA CAILLE's refractions to be considerably too great, at the same time vindicated them as fit for his instrument, because he deduced them in a manner which gave him the apparent elevation of objects above their true place by the sum of refraction and the error of his instrument, if his instrument measured the zenith distances too small, as I had concluded it did. The like remark may be applied to Dr. BRADLEY's table; for his refractions at the pole and equator, having been determined with one and the same quadrant, at one time turned to the north to observe the apparent zenith distance of the pole by means of the polar star and other circumpolar stars, and afterwards to the south to observe the apparent zenith distance of the equator, in the manner before explained, must necessarily be the true refractions, if the instrument measured the true angle; and the sum or difference of the true refractions and the errors of the instrument for these zenith distances, in case the instrument did not measure the true angle; and therefore equally proper to correct his observations, whether the total arc was just or not.

Moreover, from the two refractions thus found at the equator and pole, the refractions of the circumpolar ſtars at their paſſing the meridian above the pole were computed by Dr. BRADLEY, from the hypotheſis that the refractions at conſiderable altitudes are as the tangents of the zenith diſtances; which rule is pretty accurately true with reſpect to the real refractions, and would vary but little from the truth for the apparent refractions, which would be the ſum or difference of the true refractions and the errors of the arc, in caſe the total arc erred from the truth by a very ſmall qnantity, not exceeding 10 or at moſt 20 ſeconds. The obſerved zenith diſtances of the ſtars above the pole being corrected by the refractions thus computed, and ſubtracted from the known co-latitude, gave their true diſtances from the north pole, which added to the co-latitude gave their true zenith diſtances under the pole; and this diminiſhed by the obſerved zenith diſtance would give the refraction under the pole, or the ſum or difference of the refractions and the errors of the inſtrument belonging to their reſpective zenith diſtances; and thus his whole table would exhibit the ſum or difference of the true refraction and error of the inſtrument. Hence the latitude of Greenwich eſtabliſhed by Dr. BRADLEY, with his quadrant, as well as the latitudes of the Obſervatories at the College of Mazarine and the Cape of Good Hope, ſettled by the Abbé DE LA CAILLE with his ſextant, and the declinations of the ſtars and the obliquity of the ecliptic found by both will be very near the truth, independent of the juſtneſs of the total arcs, although their reſpective refractions may be ſuitable only to their own particular inſtruments. But, for the reaſons before given, I apprehend, the Abbé DE LA CAILLE's refractions to be much too large, and Dr. BRADLEY's to be very near the truth.

I shall now close my enquiry into the latitudes of Greenwich and Paris, and Dr. BRADLEY's and the Abbé DE LA CAILLE's refractions, by a remark naturally arising from my comparison of, and endeavours to reconcile, their observations, which I desire to submit to the consideration of astronomers, it not having, that I know of, been made before; that a table of refractions should be made for every vertical instrument from observations made with itself turned alternately north and south; and that the table, so made, applied to observations made with it, will give the true zenith distances, whether the total arc of the instrument be accurately just, or affected with a small error, or however unequally it be divided below the pole, provided the divisions are equal between themselves in the part of the instrument lying between the equator, the zenith, and the pole.

It remains to give some account of the longitude of Greenwich, or rather of the difference of meridians of Greenwich and Paris, in reply to the late M. CASSINI's doubts on the subject. This had been settled by Dr. BRADLEY at 9′ 20″, as he informed me himself, and that he had deduced it from eclipses of Jupiter's first satellite observed at both places, and that he had found it come out the same both from the immersions and emersions. This quantity had been inserted in the table of latitudes and longitudes of places, prefixed to Dr. HALLEY's tables, on the authority of Dr. BRADLEY, so long ago as the year 1749, the date of the publication of those tables, and was generally admitted by astronomers till the year 1763, when the late Mr. JAMES SHORT, F. R. S. computed it from the four transits of Mercury over the sun in 1723, 1736, 1743, and 1753, observed at Paris, London, and Greenwich, to be 9′ 16″. See Philosophical Transactions, Vol. LIII. p. 158. In

the year 1776, I requested the late Mr. WARGENTIN, the learned Secretary of the Royal Academy of Sciences at Stockholm, and Author of the improved Tables for computing the Eclipses of Jupiter's Satellites, who collected observations of them from the principal Observatories of Europe, in order to the further improvement of the tables, to inform me what difference of meridians of Greenwich and Paris resulted from my last ten years observations of the eclipses of the first satellite of Jupiter compared with those made by M. MESSIER at Paris. In the answer which he favoured me with, inserted in the Philosophical Transactions, Vol. LXVII. p. 162. he set down the result of the comparisons of eight corresponding immersions and nine emersions observed on both parts, by myself and M. MESSIER, from which he deduced the difference of meridians of the Royal Observatories of Greenwich and Paris 9′ 35″. By two corresponding immersions and nine corresponding emersions, observed at both Royal Observatories, he found 9′ 21″. From the observations made between 1761 and 1764 he found 9′ 28″. By the observations made before 1700, 9′ 21″. And, from a comparison of mine and the Parisian observations, with the intermediate help of his own made at Stockholm, 9′ 26″: and from the whole he inferred the difference of meridians to be 9′ 25″.

Twelve years having elapsed since Mr. WARGENTIN's comparison, I was desirous to see what would result from the further observations made during that time, and applied to the Comte CASSINI, the respectable heir of the late M. CASSINI DE THURY and his successor at the Royal Observatory, and to the celebrated M. MESSIER, to favour me with such of their observations of the eclipses of the first satellite of Jupiter as had been made correspondent to mine. These they immediately sent me in the

the moſt obliging manner, by which I am enabled to make further inferences concerning the difference of our meridians, as exhibited in the two following tables; in reference to which it is to be underſtood, that all the obſervations at Greenwich were made with a 46-inch achromatic teleſcope of 3,6 inches aperture, except a few otherwiſe noted as obſerved with a 6-feet Newtonian reflector, whoſe aperture is 9,4 inches; and once with a 2-feet Gregorian reflector, whoſe aperture is 4,5 inches; and once with an 18-inch Gregorian reflector, with an aperture of 4,4 inches, furniſhed with new metals of Mr. EDWARDS's brilliant compoſition, deſcribed in the Appendix to the Nautical Almanac of the preſent year, which reflects as much of the incident light as an achromatic teleſcope tranſmits; and that in making out the columns, intitled difference of meridians corrected, I have ſubtracted 7″ from the immerſions and added as much to the emerſions obſerved with the 6-feet reflector, and added 13″ to the immerſions and ſubtracted as much from the emerſions obſerved with the 2-feet reflector, to reduce them to what they ſhould have been probably obſerved at with the 46-inch achromatic teleſcope, and added 5″ to the time of the emerſion obſerved on Sept. 5, 1784 at the Royal Obſervatory at Paris, with a 5-feet reflector of DOLLOND, to reduce it to the 3½ feet achromatic teleſcope.

Difference

Difference of meridians of the Royal Observatories at Greenwich and Paris, by observations of eclipses of Jupiter's first satellite, observed at both places.

By Immersions.	Difference of meridians.	Diff. of meridians corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
	′ ″	′ ″		
1779, Jan. 11	9 22	9 29	6 F.	Limbs undulating.
18	9 38	9 45	6 F.; air a little hazy.	Air very clear.
1780, Jan. 14	9 5	9 5	Air very clear.	
Mar. 18	9 16	9 16	Air very clear.	Air hazy.
25	9 8	9 8	— —	Air a little hazy.
1785, Oct. 1	9 11	9 11	In contact with Jupiter's body.	A little hazy.
Mean of 6 imm.	9 17	9 19		

By Emersions.	Difference of meridians.	Diff. of meridians corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
	′ ″	′ ″		
1773, Nov. 1	9 10	9 3	6 F. Air a little hazy.	
1775, Feb. 15	10 44	10 37	6 F. Twilight.	Air very clear.
Mar. 17	9 43	9 36	6 F. Air a little hazy.	Air very clear.
1778, Mar. 13	10 46	10 46	—Bright moonshine.	
Apr. 12	9 46	9 46	Air hazy.	
1779, Mar. 30	10 43	10 43	— —	Air hazy.
Apr. 1	9 56	10 9	2 F. reflector. Air very clear.	
1781, May 24	9 49	9 49	Air very clear.	
31	9 8	9 8	— —	Air very clear.
1782, Aug. 29	9 27	9 27	Air very clear.	
1783, Aug. 2	8 54	8 54	Air very clear.	The sat. very near Jupiter's disk.
Oct. 26	9 45	9 45	Air very clear, but Jupiter low.	Limbs undulating much.
1784, Sept. 5	9 4	9 9	Air very clear.	5-feet reflector by DOLLOND, magnifying 450 times.
1785, Nov. 9	9 26	9 26	18 inch reflector, new metals.	
16	9 45	9 45	— —	Hazy.
Mean of 15 emer.	9 44,4	9 42		
Mean of 6 imm.	9 17	9 19		
Mean of both means	9 31	9 30½		

Before

Before the 24th of May, 1781, a 3½-feet achromatic teleſcope of DOLLOND, of 42 lines aperture, that was but an indifferent one, was made uſe of at the Royal Obſervatory at Paris. From that time a very good one was employed of the ſame ſize and aperture.

Difference of meridians of the Royal Obſervatory of Greenwich and the Hôtel de Clugny at Paris, 2″ of time Eaſt of the Royal Obſervatory, deduced from obſervations of eclipſes of Jupiter's firſt ſatellite obſerved at both places.

By Immerſions.	Difference of meridians.	Diff. of meridians corrected.	Circumſtances of the obſervations at Greenwich.	Circumſtances of the obſervations at Hôtel de Clugny.
	′ ″	′ ″		
1775, July 15	9 55	9 55	– –	Air very clear.
Aug. 7	9 6	9 13	6 F.	Air very clear.
1776, Sept. 10	9 22	9 22	Jupiter a little hazy.	
1777, Sept. 6	9 33	9 33	– –	Air very clear.
Nov. 7	9 23	9 23	Air hazy.	Air very clear.
1779, Jan. 11	10 18	10 25	6 F.	Air very clear.
18	9 23	9 30	6 F. air a little hazy.	Air very clear.
Dec. 22	8 10	8 10	Air very clear.	A little hazy.
1780, Jan. 7	9 39	9 39	Air very clear.	Air very clear.
14	9 40	9 40	Air very clear.	Air very clear.
Mar. 18	9 2	9 2	Air very clear.	Air very clear.
25	9 24	9 24		
1781, Feb. 26	8 48	8 48	♃'s limb undulates.	
Mar. 5	8 59	8 59	– –	Air very clear.
Apr. 13	9 16	9 16	– –	Jupiter ill defined.
1783, July 8	9 58	9 58	– –	Air very clear.
1786, Sept. 4	9 19	9 19	Air very clear.	
Dec. 30	9 31	9 31	Air very clear.	Very hazy.
Mean of 18imm.	9 22,5	9 23,7		

By

By Emersions.	Difference of meridians.	Diff. of meridians corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Hôtel de Clugny.
	′ ″	′ ″		
1775, Feb. 15	9 31	9 24	6 F. Twilight.	Air very clear.
22	9 1	8 54	6 F.	Air very clear.
Mar. 17	9 13	9 6	6 F. Air a little hazy.	Hazy.
1776, Jan. 26	9 15	9 15	— —	
1777, Feb. 4	8 51	8 51	Air very clear.	Air very clear.
Mar. 24	9 13	9 13	Air very clear.	Air very clear.
31	9 22	9 22	Air very clear.	
1778, Feb. 25	8 48	8 48	— —	Air hazy.
Mar. 13	9 15	9 15	— —	A little hazy.
Apr. 5	9 17	9 17	Air hazy.	Air very clear.
12	10 11	10 11	— —	Air very clear.
May 21	9 29	9 22	6 F.	Air very clear.
1780, Apr. 19	9 28	9 28	Air very clear.	
May 28	9 37	9 37	— —	Air very clear.
1781, May 31	9 10	9 10	— —	Air a little hazy.
June 16	9 25	9 25	— —	Air a little hazy.
1783, Aug. 2	9 23	9 23	Air very clear.	Air very clear.
25	9 40	9 40	Air very clear.	
Oct. 3	9 30	9 30	Air very clear.	Air hazy.
1785, Nov. 9	9 14	9 14	18-inch reflector, new metals.	
18	9 12	9 12	Air very clear.	
1786, Jan. 3	9 58	9 58	Air a little hazy.	Air hazy.
Mean of 22 emer.	9 22	9 20,7	M. Messier's achrom. telescope is of 3½ feet focus, 40 lines aperture, with magnifying powers of 70 and 140.	
Mean of 18 imm.	9 22,5	9 23,7		
Mean by both Royal Observ. west of Hôtel de Clugny	9 22 — 2	9 22 — 2		
Diff. of merid. of the Royal observatories	9 20	9 20		

Hence the difference of meridians of the two Royal Observatories, by the observations made in the Royal Observatories themselves, is 9′ 30″; and by the observations made by

M. MESSIER, at the Hôtel de Cluguy, and reduced to the Royal Observatory is 9′ 20″. The mean of both results is 9′ 25″. But if greater weight be given to the latter determination than to the former in the ratio of 2 to 1, on account of the series of M. MESSIER's observations being the most complete, the difference of meridians will be 9′ 23″.

M. DU SEJOUR, in the Memoires of the Royal Academy of Sciences for 1771, found 9′ 20″, as well from the beginning as end of the solar eclipse of 1769. M. MECHAIN, the learned editor of the *Connoissance des Temps*, informs me, that from the immersions of Celeno and Maia at the moon's limb, on March 5th last year, he has found by calculation from M. MESSIER's observations compared with mine 9′ 19″,9 and 9′ 17″,9, or by a mean 9′ 18″,9; but, by his own observations compared in like manner, he makes it a little more than 9′ 20″. He regulated his clock by corresponding altitudes; but M. MESSIER corrected his by a transit instrument, which, however, has no meridian mark. For the present, I infer, we may take the difference of meridians 9′ 20″, as being within a very few seconds of the truth, till some more occultations of fixed stars by the moon, already observed, or hereafter to be observed, in favourable circumstances, and carefully calculated, shall enable us to establish it with the last exactness. To collect and calculate such observations I have not leisure at present; but the field of calculation is equally open to the celebrated astronomers of Paris, the observations made at this place being now published annually.

The extensive geometrical operations recommended by the late M. CASSINI DE THURY, and commenced under the direction of Major-general ROY, F. R. S. by his exact measure of a base on Hounslow-Heath, may also, when completed, determine

mine the difference of meridians of Greenwich and Paris to great exactneſs. But they do not ſeem to me likely to throw any new light on the difference of latitude of the two Obſervatories, becauſe the uncertainty we are ſtill under about the true figure and dimenſions of the earth, and the irregular attractions ariſing from the irregular external figure and unequal denſity of the internal parts of the earth would prevent us from drawing any accurate concluſions, or ſuch as we could confide in, from thoſe geometrical meaſures, with reſpect to ſo large a quantity as 2° 38′ 26″ the difference of latitude; and, at all events, it muſt be leſs exact, as it is leſs direct, to determine the difference of latitude of two places from the meaſured diſtance of the two parallels compared with the length of a degree in the intermediate latitude, inferred from former meaſures of degrees, which were themſelves determined with the help of aſtronomical obſervations, than to infer it from the immediate aſtronomical obſervations made at the two obſervatories, in the manner I have already deduced it.

NEVIL MASKELYNE,
Aſtronomer Royal.

Greenwich,
February 21, 1787.

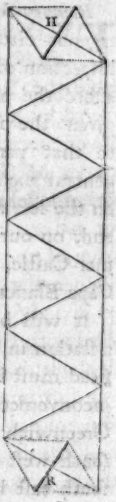
XIX. *An Account of the Mode propoſed to be followed in determining the relative Situation of the Royal Obſervatories of* Greenwich *and* Paris. *By Major-General* William Roy, *F. R. S. and A. S.*

Read February 22, 1787.

TWO years have nearly elapſed ſince an account of the meaſurement of a baſe on Hounſlow-Heath was laid before the Royal Society, being the firſt part of an operation ordered by his Majeſty to be executed for the immediate purpoſe of aſcertaining the relative ſituations of the Royal Obſervatories of Greenwich and Paris; but whoſe chief and ultimate object has always been conſidered of a ſtill more important nature, namely, the laying the foundation of a general ſurvey of the Britiſh Iſlands.

When the operation commenced in 1784, it was not doubted, that in 1786, at lateſt, we ſhould have been able to have proceeded with the ſeries of triangles from Hounſlow-Heath to the neighbourhood of Dover; but the contrivance and conſtruction of an inſtrument, new of its kind, propoſed to be made uſe of, and more particularly the nicety of its diviſion, whereby it is hoped the angles may be determined to a degree of preciſion hitherto unexampled, have required much more time than Mr.

Ramsden himself at first imagined. Without meaning to disappoint, this ingenious artist was perhaps in the outset too remiss and dilatory, and accidents having happened when the workmanship was already far advanced, which he could not foresee or prevent, the execution has thereby been greatly retarded. However, since the instrument may at present be considered as nearly finished (such parts as yet remain to be perfected being only of the smaller kind), we may fairly conclude, that early in the ensuing summer, or as soon as the weather in this country will permit, the trigonometrical operation may be begun. In this state of things, I have therefore judged that it might be proper to lay before the Society a short sketch of the mode which is proposed to be followed in fulfilling his Majesty's commands, accompanied by a very slight general map of the country, only collected from the common surveys, but still sufficient to shew nearly the disposition of the triangles that will be made use of in forming the junctions between the meridians of the two Observatories. In this business it will be understood, that I mean to adhere to such principles as have been universally received and admitted as just.



In every series of triangles where each angle is to be actually observed with the same instrument, they should, as near as the circumstances will permit, be equilateral: for were it possible to choose the stations in such a manner as that each angle should be exactly 60 degrees, the half number of triangles in the series, multiplied by the length of one side, would, as in the annexed figure, give at once the total distance; not only the sides of the

the ſcale or ladder would be perfectly parallel, but the diagonal ſteps, marking the progreſs from one extremity to the other, would be alternately ſo throughout the whole length. The firſt ſide is ſuppoſed to be found by the meaſurement of a baſe H, of about half its length; and the laſt ſide to be verified by ſuch another baſe R at the oppoſite extremity.

In any particular caſe, where only two angles of a triangle can be actually obſerved, theſe ſhould be as near as poſſible each 45 degrees. At any rate their ſum ſhould not differ much from 90°; for the leſs the computed angle differs from 90°, the leſs chance there will be of any conſiderable error in the interſection.

Romney-Marſh, from its levelneſs, as well as other advantageous circumſtances attending its ſituation, which the bare inſpection of the map will render obvious, ſeeming to me to afford the beſt baſe of verification for the laſt triangle, I have given the ſeries the ſhorteſt direction from Hounſlow-Heath to that part of Kent. The right hand ſtations occupy in general the heights which extend acroſs the Wealds. Thoſe on the left are placed on the great range of chalk hills, which end, on our ſide of the Channel, between Folkſtone and Walmer-Caſtle, and re-commence on the oppoſite ſide between Cape Blancnez and Calais.

It will be perceived, that I do not mean to make St. Paul's a ſtation in the ſuite; becauſe in that caſe Harrow and Hampſtead muſt likewiſe have been made uſe of, all three extremely inconvenient for the reception of the great inſtrument. Beſides, Greenwich Obſervatory being hidden from the country to the ſouth-weſt by the Norwood heights, and from that to the ſouth-eaſt by Shooter's-Hill, after having made the detour of Harrow and Hampſtead, and come acroſs the ſmoke of the Capital,

Capital, we ſhould ſtill have been obliged to make uſe of the two ſtations of Norwood and Shooter's-hill, without procuring ſo good an interſection of Bottle-hill, called in the common maps (I believe erroneouſly) Botley-hill, as is obtained by means of the ſtation at the Hundred-acre-houſe. But although none of the ſtations of the ſeries actually fall within London, nevertheleſs, from thoſe in its vicinity, *viz.* the Pagoda, Norwood, Greenwich Obſervatory, and Shooter's-hill, we ſhall equally have it in our power to determine accurately the ſituations of Harrow, Hampſtead, and St. Paul's, as well as many other chief ſteeples within the limits of the Capital.

Another principle I have endeavoured to adhere to, in the diſpoſition of the triangles, is this, that, after having obtained ſides in length from 12 to 18 miles, I continue them at that length as much as the circumſtances will permit; for, if they came to be reduced conſiderably below that extent, the obvious advantages of a long baſe at the outſet would be loſt, by the ſubſequent contraction towards the cloſe of the operation.

The tower of Tenterden church, being a very conſpicuous object, may be ſeen every where from the ſummit of the chalk hills, as far weſt as the river Medway. It likewiſe may be ſeen from the eaſtern extremity of the ſecond baſe, whereby the laſt triangle, *Tenterden*, *Lid*, *Allington Knoll*, is propoſed to be verified. This knoll is itſelf a very remarkable object, more acceſſible, and in other reſpects more proper too for the purpoſe of a ſtation than Lymne church ſteeple, which I had at one time thoughts of occupying. The high ground which ſeparates Romney-marſh from the Wealds of Kent, paſſes immediately behind *Ruckinge*, that is to ſay, to the north-weſtward of it, and may therefore probably prevent the top of the chalk hills from being ſeen from the weſt end of the baſe

of verification; but if *Tatterlees-Barn**, or any other point on the range near it, can be ſeen from Ruckinge, then the ſtation on the knoll, as well as that at Lymne, will become equally unneceſſary, and the triangle of verification will become *Tenterden*, *Lid*, *Tatterlees*.

It will be perceived, that I propoſe to have a ſtation on *Fairlight Head*, a land of conſiderable height, from whence there is a good view of the coaſt of France near Boulogne. From this point and Tatterlees, with the help of the Indian lights, I have no doubt of obtaining a fine interſection of the ſignal of the *Boulemberg*, a hill of ſome note behind the town of Boulogne, and one of the ſtations made uſe of by the French Academicians in the execution of their triangles. The advantages of obtaining a triangle of this magnitude, whoſe ſides are reſpectively in length about 45, 36, and 25 miles, are too obvious to require any comment.

The high chalk cliffs near Folkſtone prevent *Dover Caſtle* from being ſeen from Lid, or any where in the plain of Romney-marſh. Hence it will become neceſſary to form two ſmall triangles to the northward of Tatterlees, in order to obtain an interſection of one of the turrets of the keep of that caſtle. Of the center of the keep, it will be perceived, by the ſtrong dotted lines, that the French Academicians have procured, from their ſtations at *Calais*, *Blancnez*, and *Audinghen*, an acute interſection *(de la Groſſe Tour de Douvres)* making in the whole an angle of 28° 16′ 20″.

The points which are obviouſly the beſt for connecting our triangles with thoſe of our neighbours, are the Boulemberg, Blancnez, and Calais, provided we could by any means obtain

* Tatterlees Barn, in Packe's Map of East Kent, on the ſummit of the chalk hills, 727 feet above the ſea.

as good an intersection of the last as we are certain of getting of the two first; but the breadth of the range of chalk hills being little different on our side from what it is on theirs, by confining ourselves to such a base as they will afford us, we cannot any way obtain an intersected angle at Calais greater than about 29° or 30°.

Thinking that possibly from St. Peter's church in the isle of Thanet the tower of *Notre Dame* at Calais may be seen, I have extended dotted triangles into that part of Kent; because, if the united heights should not be sufficient to raise the top of the tower above the curvature of the sea, which is the only thing to be doubted, we are always certain, that the signal of *Blancnez* may, by means of the Indian lights, be easily seen, since the whole range of chalk hills behind Calais are discovered with the naked eye from the isle of Thanet, when the weather is tolerably clear.

Having in this manner ascertained the relative situations, with regard to the coast of England, of three points on the coast of France, forming a triangle whose sides and angles we already know from their trigonometrical operations, we shall in like manner be enabled to determine the situation of the point M near Dunkirk, where the meridian of the Royal Observatory of Paris intersects a line drawn from the great tower of Dunkirk to that of *N. D.* at Calais. (See Tab. IX.) The distance MP, on the meridian of Paris, will then be had; and that being added to 133417 fathoms, the distance of M northward from the Royal Observatory, we shall have the total terrestrial arc, comprehended between the parallels of the two Observatories, answering to an arc in the heavens of 2° 38′ 26″, or a difference of latitude between 51° 28′ 40″ and 48° 50′ 14″.

In like manner the distance from Greenwich to P, on the parallel of Greenwich, will then be readily computed, answering to the difference of longitude between the two Observatories; which, as far as can be judged from the map of Kent, corrected for the error in the direction of its meridian, amounts to about 2° 20′ 20″, supposing always that no uncertainty remains with regard to the position of the point M. But here some remarks become necessary, which may probably suggest to the Academy of Sciences, that a further investigation of this matter may be needful on their part.

By referring to the 57th page of the first part of M. Cassini's Book (*La Méridienne vérifiée*), it will be seen, that Dunkirk, by one series of triangles, is eastward from the meridian of Paris 1426.53, and by another 1414.29 toises, whereof the mean is 1420.41, equal to 1514 fathoms. This difference of 6½ fathoms, or little more than half a second of longitude between the mean and extreme places of M, is certainly very inconsiderable. But in the 60th page, where, in verifying the meridian of Paris, by the comparison of the angle that *Broulezele* makes with the meridian of Dunkirk, and the angle of convergence of one meridian to the other, a difference of 21 seconds between 10° 16′ 13″ and 10° 16′ 34″, is alledged to be almost insensible, we do not think to be a conclusion so unexceptionable. This, however, is not the only cause of uncertainty with regard to the just position of the point M: one of more importance arises, from the difference that is found by two sets of triangles in the angle of intersection of the meridian of Paris, with a line drawn through M from the tower of Dunkirk to that of Calais.

Thus, by p. 53. and 56. of the first part of M. Cassini's book, Dunkirk being the station, Broulezele makes an angle with

with the meridian of 10° 18′ 25″ towards the ſouth-weſt: and the angle between Broulezele and Hondſcote being 78° 11′ 42″, their difference 67° 53′ 17″ is the angle that Hondſcote is ſouth-eaſt from the meridian; wherefore the complement of this laſt angle to 180°, *viz.* 112° 6′ 43″ is the angle that Hondſcote makes with the meridian of Dunkirk produced northward. By p. 166. of the ſecond part, Dunkirk being the ſtation, the angle between Hondſcote and Mont-Caſſel is ſhewn to be 51° 7′ 15″; that between Mont-Caſſel and Watten 42° 6′ 35″; and by p. 167. that between Watten and Calais is 51° 40′ 20″. The ſum of theſe three angles is 144° 54′ 10″, from which deducting 67° 53′ 17″ the angle that Hondſcote is ſouth-eaſtward from the meridian, there remain 77° 0′ 53″ for the angle of Calais ſouth-weſtward from it; and the complement of this angle to 180°, *viz.* 102° 59′ 7″, becomes the angle that the meridian of Dunkirk produced northward makes with a line drawn through M to Calais: to which laſt adding the angle of convergence of one meridian to the other 1″ 50″½, correſponding to the diſtance of 1514 fathoms, equal to 1′ 29″½ of a great circle, we ſhall have 103° 0′ 57″½ for the angle which the meridian of Paris produced northward from M makes with the line joining Dunkirk and Calais.

Again, by p. 63. of the third part of M. Cassini's book, Dunkirk being the ſtation, the angle that Gravelines makes with the meridian ſouth-weſtward is 72° 11′ 48″; and by p. 12. of the ſaid third part, the angle between Watten and *N. D.* Calais is 51° 39′ 50″: alſo that between Gravelines and Calais is 56° 42′ 0″. Now the difference between theſe two laſt 4° 47′ 50″ being added to 72° 11′ 48″, we ſhall have 76° 59′ 38″. and its complement 103° 0′ 22″, for the angles that the meridian of Dunkirk makes with the line drawn from thence through the point M to Calais: to which laſt angle adding

the former convergence 1′ 50″¼, we have 103° 2′ 12″¼ for the angle that the meridian of Paris produced northward from M makes with the said line; but by the former set of angles it was found to be only 103° 0′ 57″¼, wherefore the difference is 1′ 15″.

From M. Cassini's book it appears, that Dunkirk is north from Paris 125515.25 toises, which make 133768 fathoms; and the point M being south from the tower of Dunkirk 351 fathoms, there remains for the distance of M northward from the Royal Observatory 133417 fathoms. Now, with this distance as radius, the value of an angle of 1′ 15″ is 48¾ fathoms, equal to 4″.34‴ of longitude. Thus the point M, instead of being westward from Dunkirk 1514 fathoms, will, by the last set of angles, only be removed from it 1465¼ fathoms: wherefore the difference between the mean and extreme places of M, in this way of considering it, will amount to 24¾ fathoms, about four times as much as that resulting from the comparison stated in the 57th page. In the parallel of Greenwich the extreme difference will amount to 58.4 fathoms, or about 5½ seconds of longitude, not much more than one-third part of a second of time.

In this sort of uncertainty, with regard to the precise point of intersection of the meridian of the Royal Observatory of Paris with the line joining Dunkirk and Calais, the only thing that can be done on our part, is to consider the mean position of M as just, that is to say, to suppose it to be 1514 fathoms westward from the great tower of Dunkirk, and having connected it with the British triangles, to shew then what angle its meridian will make with the line drawn from Dunkirk to Calais.

Comparison

Compariſon of the celeſtial arc of the meridian, comprehended between the parallels of Greenwich *and* Perpignan, *with the correſponding portions, meaſured and computed, of the terreſtrial arc of the ſaid meridian, between* M *and* Perpignan.

In the conſideration of this matter it is to be obſerved, that M. CASSINI has divided the celeſtial arc between the parallel of Dunkirk, or, which is the ſame thing, between the parallel of M and Perpignan, into four principal ſections; *viz.* that from M to Paris, from Paris to Bourges, from Bourges to Rodés, and from Rodés to Perpignan; aſſigning to each ſection the meaſured portion of the correſponding terreſtrial arc, reſulting from the triangles of the meridian.

By pages 110, 111, 112, of the firſt part of the book it appears, from the mean obſerved zenith diſtances of four ſtars, that the arc of the heavens, between the parallels of Dunkirk and Perpignan, contains — — — — — — 8 20 2 26

And from the mean of the obſervations of a like number of ſtars, the parallel of Rodés is diſtant from that of Dunkirk, — 6 50 51 14

Wherefore Rodés is north from Perpignan — 1 39 11 12

Bourges, by the mean zenith diſtances of two ſtars, is ſouth from Dunkirk, — — — — — — 3 56 59 55

Wherefore Bourges is north from Perpignan, — 4 23 2 31

And by another mean, in p. 112. it is — — 4 23 2 35

The mean of which two means gives for the diſtance of their parallels, — — — — — 4 23 2 33

Perpignan

	°	′	″	‴
Perpignan is ſouth from the Royal Obſervatory at Paris, by the mean zenith diſtances of four ſtars,	6	8	11	52
Hence Paris is ſouth from Dunkirk	2	11	50	34
Rodés is ſouth from Paris, by the mean zenith diſtances of two ſtars	4	28	59	45
Hence Perpignan is ſouth from Rodés,	1	39	12	7
And by a former reſult it was found to be,	1	39	11	12
Hence the mean of the two means gives, for the diſtance of the parallels of Rodés and Perpignan,	1	39	11	28

The tower of the great church at Dunkirk is northward from what was the ſtation of the ſector 84¼ toiſes, equal to 89.8 fathoms, which correſpond to an arc in the heavens of	0	0	5	19
Wherefore the tower is north from Paris,	2	11	55	53
But the point M is ſouth from the tower 351 fathoms, which anſwer to an arc in the heavens of	0	0	20	45
Hence the point M is northward from the Royal Obſervatory at Paris,	2	11	35	8

Now, from theſe *data*, together with the latitude of the Royal Obſervatory at Greenwich 51° 28′ 40″, and that of Paris 48° 50′ 14″, we ſhall have the latitudes of the ſeveral ſtations between Greenwich and Perpignan, with their differences, or the celeſtial arcs comprehended between them, as underneath.

Stations.	Latitudes. °	′	″	‴	Diff. or celeſtial arcs. °	′	″	‴
Greenwich Royal Obſervatory,	51	28	40					
Point M near Dunkirk,	51	1	49	8	0	26	50	52
Paris Royal Obſervatory,	48	50	14		2	11	35	8
Bourges,	47	5	4	41	1	45	9	19
Rodés	44	21	13	36	2	43	51	5
Perpignan (*St. Jaumes*)	42	42	2	8	1	39	11	28

This latitude of Perpignan 42° 42′ 2″ 8‴ is what reſults from the immediate compariſon of the lengths of the celeſtial arcs

arcs, as determined by the zenith diſtances of ſtars, taken with a ſector of ſix feet radius, and where the obſervations are ſo nearly conſiſtent among themſelves, as to leave little doubt of their accuracy; but in the 290th page of M. CASSINI's book, ſo often quoted, as well as in the 170th page of his *Deſcription Géographique de la France*, publiſhed in 1783, the latitude of Perpignan is given 42° 41′ 55″, which is 7″ 8‴ leſs than that deduced from the obſervations, without any reaſon that I can perceive being aſſigned for the reduction.

Perpignan, the ſouthernmoſt ſtation of the meridian line extending from Dunkirk through the whole kingdom of France, is ſituated at no great diſtance from the bottom of the Pyrenean mountains, where that lofty range ends at the Mediterranean ſea. M. DE LA CAILLE was of opinion, that the plummet of the ſector muſt have been affected by the attraction which it would ſuffer from that cauſe; a ſuppoſition which, nevertheleſs, has been doubted, ſince the obſervations made in this country on the attraction of *Schehallion*: for by theſe it appeared, that the effect, although ſenſible, was but ſmall, even when the ſector was placed as near as poſſible to the oppoſite ſides of the mountain. It is indeed true, that the *Canigou*, the higheſt of the Pyrenean range, being ſituated obliquely to the meridian, and at a conſiderable diſtance from Perpignan, would not probably occaſion much deviation in the plummet; yet, on the other hand, when we compare the very trifling quantity of matter in Schehallion with the immenſity of the maſs in the Pyrenees, in the direction of the meridian, I cannot help being of M. DE LA CAILLE's opinion, that the plummet of the ſector would be ſenſibly affected, that is to ſay, it would be drawn to the ſouthward out of its perpendicular direction, and would thereby give the zenith diſtance of the

pole,

pole, or any other northern ſtar, too little, and conſequently a latitude too great. Until triangles ſhall have been extended beyond the Pyrenees, and the ſector placed on the ſouth ſide of the range, the quantity of this attraction (by its double or counter-effect) cannot poſſibly be aſcertained. I will, however, only ſuppoſe it to have been 10″ 8‴ to be deducted from the latitude of Perpignan, which will then become 42° 41′ 52″, only three ſeconds leſs than that aſſigned to it in M. Cassini's two books before mentioned. Thus the arc between Rodés and Perpignan will be 1° 39′ 21″ 36‴, and the total celeſtial arc between Greenwich and Perpignan will be 8° 46′ 48″, as may be ſeen by attending to the four columns towards the left-hand of the annexed table of compariſon.

With regard to the correſponding terreſtrial arc, under which head are arranged the eleven columns towards the right-hand of the table, it is to be obſerved, that various meaſurements have at different times been made in different latitudes of the lengths of the degrees of the meridian, for the purpoſe of obtaining, within certain limits at leaſt, the true figure and dimenſions of the earth. The moſt eſſential operations of this ſort, as having been executed with moſt care, with the beſt inſtruments, and at the greateſt diſtances from each other, have all been done within theſe laſt forty or fifty years; namely, in Peru under the equator, in middle latitudes in France and Italy, and in Lapland near the polar circle. The attraction of mountains, and unavoidable errors in the execution, will ever prevent juſt concluſions from being drawn from the compariſon of meaſurements made too near each other. Theſe laſt will always be found to differ more or leſs among themſelves. Sometimes even the reſults may become abſurd or contradictory. In caſes of this ſort, a mean of ſeveral ſhould no doubt be

taken for a mean latitude, unleſs there ſhould be ſufficient grounds for rejecting any from the number, as differing too much from the others. Hence it is, that philoſophers are not yet agreed in opinion with regard to the figure of the earth; ſome contending, that it has no regular figure, that is to ſay, not ſuch as would be generated by the revolution of a curve around its axis. Others have ſuppoſed it to be an ellipſoid; regular, if both polar ſides ſhould have the ſame degree of flatneſs; but irregular, if one ſhould be flatter than the other. And, laſtly, ſome ſuppoſe it to be a ſpheroid differing from the ellipſoid, but ſuch as would be formed, nevertheleſs, by the revolution of a curve around its axis; although in this caſe too one polar ſide may not be ſimilar to, but more or leſs oblate than the other.

In order, therefore, to put this matter in its true light, and to enable every one to judge, by ſimple inſpection only, which of the theories agrees beſt with actual meaſurement, I have computed on ten different *hypotheſes*, and arranged in their order, the lengths of the arc between Greenwich and Perpignan; as alſo ſome other chief properties of each figure, which laſt fill up the ſpace towards the bottom of the table. This mode of collecting the reſults ſeemed to me to be the moſt diſtinct that could be followed, to avoid that perplexity which muſt for ever occur in referring back to, and comparing, many numbers together, computed on different ſyſtems, when the whole are not placed before the eye at once.

The firſt of the eleven columns, or that which comes next to the celeſtial arc, contains the meaſured portions of the correſponding terreſtrial arc, as far as they have yet been executed. The blanks at top cannot be ſupplied, until we ſhall

have determined the length MP, in the map, being the ſpace comprehended between the parallels of M and Greenwich.

In the ſecond column are arranged the computed dimenſions appertaining to the earth as a ſphere, ſuppoſing its ſemi-diameter to be a mean between the longeſt and ſhorteſt of M. Bouguer's ſecond ſpheroid. It is from the magnitude of this ſphere that I compute the degrees of a great circle for the ſides of ſpherical triangles. By adverting to the errors or differences between the meaſurement and computation, in their reſpective places, it will obviouſly appear that the earth differs very conſiderably from a ſphere: for although the arc M Perpignan of $8^{\circ}\frac{1}{4}$ only exceeds the truth by 609 fathoms; yet an arc of equal length at the equator, *viz.* $8^{\circ}.33 \times 374.6$, would give an exceſs of 3120 fathoms; and at the polar circle $8^{\circ}33 \times 335.2$ would give a defect of 2792 fathoms.

After the ſphere follow ſeven ellipſoids of different degrees of oblateneſs, from the firſt, whoſe ſemi-diameters have to each other the ratio of 179.047 to 178.047, to the ſeventh, where it is only that of 540 to 539. On the principles which have ſerved as the foundation of the firſt and ſecond, it will be neceſſary to make ſome remarks; but as to the others, a few words will ſuffice for each.

With regard to the firſt ellipſoid, ſuppoſing the earth to be homogeneous, it is well known, that the ratio of its ſemi-diameters may be found, by comparing with each other the lengths of the pendulums that vibrate ſeconds in different latitudes; which lengths are deduced from the ſeconds of acceleration, that the pendulum, ſo adjuſted, and unalterably fixed as to length, at the equator, would perform in 24 hours, on being ſucceſſively tranſported to different latitudes, as far as the pole, where the force of gravity being the greateſt, the acceleration would likewiſe be the greateſt. The calcu-

lations for this purpose were first made soon after * Lord MULGRAVE's return from his Voyage towards the North Pole in 1773.

* The most northern experiments hitherto made with the pendulum, are those at Spitzbergen, in latitude 79° 50′, whereof an account has been given in the Voyage towards the North Pole in 1773. The machine made use of on that occasion belonged originally to the celebrated watch-maker, Mr. GRAHAM, and was lent for the purpose by its present proprietor, Mr. CUMMING, who has since obligingly permitted it to remain for several years, at two different times, in my possession, where it now is. Soon after the return from Spitzbergen, I ascertained its rate of going by my observatory clock, for a great while together, in very different temperatures; and thereby found, that the variation from heat and cold, namely, $\frac{1}{10}$ths of a second for each degree of FAHRENHEIT, was considerably more than had been allowed for it, in determining the acceleration from London to Spitzbergen. About this time likewise, Dr HORSLEY discovered that an error had been committed by the astronomer (the late Mr. ISRAEL LYONS) employed by the Board of Longitude on that Voyage, in calculating the spherical triangle for the correction of the time between the 16th and 17th of July, on account of the obliquity of the transit-instrument. From Dr. HORSLEY's printed letter on this subject in 1774, it appears, that the difference from that cause amounted to 37 seconds in time, as given by the observation with the instrument and by the watch. And since there was reason to suspect, that the telescope had by accident been moved (one could not tell how much), it was judged safest to adhere to the acceleration 71″.08 as given by the watch. The correction for the greater contraction of the steel rod, I found to be 2″.58 to be subtracted, wherefore the acceleration from London to Spitzbergen became 68″.5, and that from the equator to London being 156″, the total acceleration from the equator to Spitzbergen consequently was 224″.5. Now, supposing the length of the pendulum at the equator to be, as M. BOUGUER made it, just 38.9949 inches, we shall have its length at Spitzbergen 39.1978 inches; and thence the ratio of the semi-diameters of the earth, considered as an homogeneous ellipsoid, will be that of 193.1 to 192.1, instead of 183.7 to 182.7, which the acceleration 72″.7 uncorrected would have given. And here it seems necessary to mention some other mistakes of computation, inadvertently fallen into by the astronomer, in deducing the ratios of the semi-diameters of the earth, as stated in the 179th page of the Voyage to the North Pole, not hitherto noticed that I know of.

1773. I am aware that experiments with the pendulum have not yet been made with that accuracy that the delicacy of their nature

With the accelerations, indicated in that page as arising from different experiments, or depending on the ſyſtems of different philoſophers, the length of the Spitzbergen pendulum, and conſequently the ratios, ſhould have ſtood as in the annexed table, with which it will be perceived they differ very conſiderably. Sir ISAAC NEWTON's ratio is placed laſt, as probably differing moſt from the truth.

Experiments.	Acceleration			Length of the Spitzberg. pendulum.	Ratio of the ſemi diameters of the earth.
	From the equator to London.	From London to Spitzberg.	Total.		
			″ ‴	In.	
Said to be, but erroneouſly, Sir I. NEWTON's	156″	+ 66.9	222.9	39.1964	194.5 to 193.5
Mr. CAMPBELL's		+ 76.6	232.6	39.2052	186.5 to 185.5
M. MAUPERTUIS's		+ 86.5	242.5	39.2141	178.8 to 177.8
Ld MULGRAVE's correct.		+ 68.5	224.5	39.1978	193.1 to 192.1
Sir ISAAC NEWTON's		+ 32.4	188.4	39.1652	230. to 229.

In this manner the lengths of the pendulum having been found for all the latitudes where the beſt or moſt conſiſtent experiments had been made on its acceleration, and theſe lengths having been ſucceſſively compared with each other from Spitzbergen to the equator, 119 reſults in the whole were produced. But as this number comprehended the compariſons of thoſe at the Cape of Good Hope and the iſle of France, both in ſouth latitude, theſe being thrown out, as well as the Porto Bello pendulum, and ſome few others more irregular than the reſt, there remained at laſt 75 reſults, the arithmetical mean of which gave the ratio 179.047 to 178.047, as mentioned in the text.

In caſe Mr. CUMMING's machine ſhould at any time hereafter be employed in the ſame ſort of experiments, it may be proper to obſerve, that the diameter of the braſs ball is 3.906 inches, and its weight 63726 Troy grains. The weight of its bulk of mercury is 106980 Troy grains. Hence the weight of mercury is to the weight of this ball of braſs, in air of the heat of 62° of FAHRENHEIT, as 1.678752 to 1; and the weight of this braſs to air is as 7673 to 1. The experiments for this purpoſe, in which Dr. GEORGE FORDYCE aſſiſted, were made in the houſe of Mr. ALCHORNE, at his Majeſty's Mint in the Tower, on the

nature ſeems to require, and which at ſome future period ſhould, and probably will, be undertaken on a more extended plan, with the very beſt machines that can be conſtructed for the purpoſe. Many ages may elapſe before meaſurements of any conſiderable portion of the ſurface of the earth can be made in very high ſouthern latitudes, ſo as to determine, with any tolerable degree of exactneſs, whether the ſouthern ſemi-ſpheroid be ſimilar to the northern, ſuppoſing both to be figures of revolution. But by good experiments with the pendulum alone, eaſily repeated in all latitudes, the ratio of the ſemi-diameter of the equator to the ſemi-axis on both ſides may be readily obtained. In the mean time I thought it might be uſeful to ſhew the reſult of a compariſon of the moſt conſiſtent experiments of that ſort, that have hitherto been made in different latitudes, after having applied to thoſe at Spitzbergen certain corrections, which ſeemed neceſſary, as further explained in the note. Thus it appears, that the arithmetical mean of 73 compariſons between Spitzbergen and the equator gives the

the 8th of April and 6th of June, 1776; and the beam made uſe of, with 15 lbs. weight in each ſcale, was true to three grains.

The machine, in its preſent ſtate, although better fitted for experiments than it was originally, ſince it has a wheel added, whereby it regiſters its own time, and now goes for 12 hours without winding up, is no longer the ſame that performed at Spitzbergen: for in the interim of the two times it has been in my poſſeſſion, the ſteel rod having by accident been broken, Mr. CUMMING ſubſtituted another, as like to the former as poſſible with regard to ſize, but which will, in all probability, be ſuſceptible of a different expanſion and contraction from the effects of heat and cold. In the application of the machine, I have uſually loaded the top of it with about 24 lbs. weight of lead, in order to render the center of ſuſpenſion as little liable to motion as poſſible: yet, notwithſtanding this precaution, a pointed plummet, ſuſpended to the top of the frame, has a ſmall degree of counter-vibration to that of the ball, which no doubt muſt produce ſome effect.

ratio

ratio of the ſemi-diameters formerly mentioned 179.047 to 178.047. On this hypotheſis the arc MP ſhould contain 27350 fathoms. The error on the total arc M Perpignan amounts to 2078 fathoms. M. BOUGUER's degree at the equator being adhered to, the 45th of latitude will exceed the truth 216, and that at the equator 148 fathoms,

The ratio of the ſemi-diameters of the ſecond ellipſoid, which comes now to be ſpoken of, has been obtained by the compariſon of ſuch meaſured lengths of the degrees of the meridian in different latitudes, as have been found to be moſt conſiſtent with each other. Our countryman, Mr. NORWOOD, was the firſt, of late times, who made any attempt of this ſort. But the meaſurement, executed by him in the year 1633, between London and York, has no pretence to exactneſs, ſince he himſelf tells us, *that when he did not meaſure, he paced!* Beſides, his degree is as great, or even greater, than that in Lapland; and theſe are ſurely ſufficient reaſons for rejecting it from the compariſon. The degree meaſured by M. LIESGANIG in latitude 45° 57′, in that part of Poland lately fallen to the ſhare of the Emperor and annexed to Hungary, being ſo much ſhorter than degrees to the ſouthward of it, gives grounds to ſuſpect, that ſome error had crept into that operation, or that the plummet had been affected by the attraction of neighbouring mountains, and therefore is not made uſe of on the preſent occaſion. M. DE LA CAILLE's degree at the Cape of Good Hope, being in ſouth latitude, and ſo much greater than thoſe of the ſame height in northern latitudes, is improper likewiſe to be brought into the compariſon, leſt the difference may have ariſen from a diſſimilarity in the two polar ſides of the ellipſoid. The degree meaſured in the north of France, compared with that in Auſtria, coming out abſurd,

abſurd, it has been judged beſt to take a mean between them for a mean latitude. In like manner the latitudes of the two Italian degrees differing but little from each other, a mean length has been taken between them for a mean latitude. Accordingly the latitudes and the meaſured lengths of the degrees which, in the ſecond ellipſoid, have been compared together, will appear as underneath:

Obſervers names.	Countries.	Latitudes.		Meaſured lengths.
		°	′	
Bouguer,	Peru, – –	0	0	– 60484.5
Mason and Dixon,	Maryland, –	39	12	– 60628.5
Boscowich,	Italy, – 43° 0′	43	52	60725.5
Beccaria,	Piedmont – 44 44			60821.3 60773.4
Cassini, &c.	Middle of France, –	45	0	– 60777.6
Liesganig,	Auſtria, – 48 43	49	3	60839.4
Cassini, &c.	North of France, – 49 23			60826.6 60833.0
Maupertuis, &c.	Lapland, –	66	20	– 61194.3

Now theſe ſix degrees, being ſucceſſively compared with each other, fifteen reſults are thereby obtained, whereof the arithmetical mean gives for the ratio of the ſemi-diameters of the ellipſoid that of 192.483 to 191.483. By adverting to the table, it will further appear, that the arc MP ſhould be in length 27331 fathoms. The arc M Perpignan exceeds the truth 1758, the 45th of latitude 180, and that at the polar circle near 88 fathoms.

The ratio of the ſemi-diameters of the third ellipſoid 216.06 to 215.06 is obtained by adhering to the meaſured lengths of the degrees at the equator and polar circle. According to this hypotheſis the arc MP ſhould contain 27301 fathoms. The arc M Perpignan exceeds the truth 1288, and that at the 45th of latitude more than 128 fathoms.

The

The ratio of the ſemi-diameters of the fourth ellipſoid 222.55 to 221.55 is the ſame, as may be ſeen by referring to the table, with that aſſigned by M. BOUGUER to his firſt ſpheroid, where the increments to the degrees of the meridian above that at the equator are as the ſecond power or ſquares of the ſines of the latitudes. It was intended chiefly to ſhew how ſmall the difference is between the magnitudes and nature of the curves of the two figures. The arc MP ſhould contain 27294 fathoms. The arc M Perpignan errs in exceſs 1177 fathoms. The 45th degree exceeds the truth 116 fathoms; and that at the polar circle falls ſhort of the meaſured length 21 fathoms: M. BOUGUER's degree at the equator being adhered to as the ſtandard.

The ratio of the ſemi-diameters of the fifth ellipſoid, 230 to 229, is that aſſigned to the earth by Sir ISAAC NEWTON. On this hypotheſis the arc MP ſhould contain 27241 fathoms. The arc M Perpignan only exceeds the truth 202 fathoms, becauſe the 45th degree of the meridian is here adhered to as the ſtandard length. But then the degree at the equator falls ſhort of the meaſurement 102 fathoms, and that at the polar circle 146¼; wherefore, an arc of 8°¼, in the firſt caſe, would be defective 850, and in the laſt 1220 fathoms.

The ratio of the ſemi-diameters of the ſixth ellipſoid, 310.3 to 309.3, is obtained by adhering to the meaſured lengths of the degrees at the equator and 45th of latitude. The arc MP ſhould contain 27220 fathoms. The arc M Perpignan only exceeds the truth 131 fathoms; but on this hypotheſis, the degree at the polar circle would be defective near 217 fathoms, and conſequently on 8°¼ the error would be 1807 fathoms.

The ſeventh or laſt ellipſoid, being that of the leaſt flattening, has for the ratio of its ſemi-diameters 540 to 539. The arc MP ſhould contain 27206 fathoms. The 45th degree of latitude being adhered to as the ſtandard, the arc M Perpignan would only exceed the truth by 46 fathoms; but, on the other hand, the degree at the equator erring in exceſs 124¼ fathoms, and that at the polar circle being defective near 303; therefore, in the firſt caſe, the error on 8°¼ would be 1037, and in the laſt 2524 fathoms. Hence it is obvious, that the arcs of an ellipſoid, however great or ſmall the degree of its oblateneſs may be, will not any way correſpond with the meaſured portions of the ſurface of the earth: for if we retain the length of M. Bouguer's degree at the equator as the ſtandard, and make the ellipſoid extremely flat, as in N° 1. the figure will become too prominent in middle latitudes, that is to ſay, the curve will riſe above the real ſurface of the earth, and, in proportion to the exceſs of the radius, will always give degrees that exceed the meaſured length. On the contrary, if we give the ellipſoid a ſmall degree of flatneſs, as in N° 7. and adopt the meaſured length of the 45th degree as the ſtandard, the meaſured and computed arcs will nearly agree in middle latitudes; but at the equator the curve will riſe very conſiderably above the ſurface, and will thereby give degrees that are too great; while at the polar circle it will fall below it, and give degrees that are too little in the proportion of about 2¼ to 1 compared with the error at the equator. From all which we may conclude, that the earth is not an ellipſoid.

The two columns towards the right-hand of the table, contain the arcs of two ſpheroids differing from the ellipſoid. The firſt is that adopted by M. Bouguer as his firſt hypotheſis, where the increments to the degrees of the meridian

above that at the equator follow the ratio of the ſecond power or ſquares of the ſines of the latitudes, and to which he has ſuited his firſt table of degrees, N° 32. p. 298. This ſpheroid differs but inſenſibly, as has been already mentioned, from the fourth ellipſoid. They have both the ſame ſemi-diameters; but the arcs of the ſpheroid being ſomewhat longer than thoſe of the ellipſoid, the former thereby becomes, in a trifling degree, more prominent in middle latitudes. On this hypotheſis the arc MP ſhould be in length 27295 fathoms; M Perpignan exceeds the meaſurement 1196 fathoms; and the degree at the equator being adhered to as the ſtandard, the 45th errs in exceſs 118, while that at the polar circle is defective only 20 fathoms.

The ſecond ſpheroid is that whereon M. Bouguer founded his ſecond hypotheſis, which ſuppoſes the increments to the degrees of the meridian, above that at the equator, to follow the ratio of the fourth power or ſquared ſquares of the ſines of the latitudes, and to which he has adapted his ſecond table of degrees N° 38. p. 305. It will be perceived, that the ratio of the ſemi-diameters of this ſpheroid, *viz.* 179.4 to 178.4 differs little from that appertaining to the firſt ellipſoid; but here the curve falling conſiderably within, that is to ſay, being leſs prominent than the ellipſoid in middle latitudes, the arcs are thereby contracted in ſuch a manner as to agree within 5 fathoms with the meaſured length of the meridian of France, in an extent of about 8°¼, comprehended between M near Dunkirk, and Perpignan ſituated at the bottom of the Pyrenean mountains. By inſpection of the table it will further appear, that the errors in the ſeveral ſections of this arc are not only ſmall, but they are ſometimes *plus* and ſometimes *minus*, a never failing proof that, as far as our preſent *data* will enable

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us to judge, the figure here aſſigned to the earth, notwithſtanding what has been alledged to the * contrary, is exceedingly near the truth. According to this hypotheſis, the diſtance MP on the meridian of Paris, which is yet to be determined by our trigonometrical operations, ſhould contain 27243 fathoms, being only 35 fathoms leſs than what is given by the mean of the ſeven different ellipſoids, a ſpace not amounting quite to 2″ of latitude. The reſult of the meaſurement of this ſpace, anſwering to an arc in the heavens of 26′ 50″ 52‴ of latitude, will be a further confirmation, or otherwiſe, of the juſtneſs of the theory. The degree at the equator being adhered to as the ſtandard, it will be ſeen from the table, that the 45th is defective 37.6, while that at the polar circle errs in exceſs 9.4 fathoms.

* Mr. J. Klosterмann, Inſpector of the Corps of Pages at St. Peterſburg, in his manuſcript Memoir, ſome time ſince tranſmitted to the Royal Society, to the Academy of Sciences at Paris, and alſo to that at Gottingen, has endeavoured to ſhew, that the French trigonometrical operations are extremely erroneous. It would ſeem, nevertheleſs, that he has attempted to prove too much. He ſhould certainly have confined his criticiſm to the triangles of the meridian only, which are diſtinguiſhed from the others in M. Cassini's book, by being printed in larger characters, without drawing concluſions from very acute angles, which, although inſerted in the general regiſter, were not made uſe of in the determination in queſtion. But as the Royal Academy of Sciences will, no doubt, vindicate the credit of their own operations, I ſhall only further remark, on the Literary News from Gottingen (*Nachricht aus den Göttingiſchen Anzeigen von gelehrten Sachen*, 117 *Stück*, 1785), which accompanied the ſaid Memoir, and where it is ſaid, "That M. Bouguer's hypotheſis of the 4th power fell to the "ground, ſo ſoon as other degrees were meaſured than thoſe on which he had "founded it," that I confeſs myſelf to be quite of a different opinion, not doubting, that when the compariſon is fairly drawn between this and every other ſyſtem that has hitherto been ſubmitted to the conſideration of the public, M. Bouguer's will be found to be juſtly entitled to the preference, which I have here endeavoured to give it. His works ſhew, that he was a man of very ſuperior abilities, eminent as a mathematician, and perhaps the beſt practical one that ever exiſted.

Beſides theſe two ſpheroids of M. BOUGUER, I had computed the arcs of another between the two, where the increments to the degrees of the meridian were in the ratio of the fractional power $3\frac{99}{100}$ of the ſines of the latitudes. Thus the computed agreed accurately with the meaſured dimenſions in three principal parts of the earth's circumference, namely, at the equator, 45° and 66° 20′ of latitude, at the ſame time that the arc MP contained 27257 fathoms; but the labour of computation being hereby greatly augmented, and the error on the total arc between M and Perpignan amounting to 290 fathoms in exceſs, this ſyſtem did not ſeem to me to deſerve to be put in competition with the ſimplicity of that of M. BOUGUER, who indeed, for the ſame reaſon, preferred the fourth to the fractional power $3\frac{1}{11}$ths, which he tells us was that which ſtill came nearer the truth. In ſhort, it muſt be from the reſults of future operations executed in very high latitudes, and the meaſurement of degrees of longitude on the equator, that we can hope to have ſufficient authority for any correction or a melioration of the ſyſtem of M. BOUGUER.

Differences of longitude.

Hitherto there has been no particular reference to ſome lines at the bottom of the table, containing the computed lengths of degrees of longitude on each hypotheſis, in three different latitudes, namely, the equator, 43° 32″ and 51° 28′ 40″. No meaſurements of degrees of longitude, as far as I know, have ever been executed with ſufficient care and accuracy, except that in the ſouth of France, as mentioned in the 105th and 106th pages of M. CASSINI's book, which was determined by the

the repeated exploſions of gunpowder in the open air, and found to contain 41618 toiſes, equal to 44354.4 fathoms. By attending to the table it will be ſeen, that the error in exceſs of M. BOUGUER's theory, on the length of this degree trigonometrically meaſured, amounts only to 19 fathoms, which is little more than $\frac{1}{10}$th part of a ſecond of time.

In fixed Obſervatories, where able aſtronomers have been for many years employed in repeating their obſervations of the heavenly bodies, it ſeems ſurpriſing, that any doubt ſhould remain with regard to what is called the aſtronomical difference of longitude, or, in other words, the difference of time between them; yet it has been alledged, that an uncertainty of this ſort exiſts, even with regard to the ſituation of Greenwich and Paris, which, reckoned by its extremes, extends to about 10 or 11 ſeconds, anſwering in the latitude of Greenwich to the enormous difference in ſpace of between 1600 and 1700 fathoms! But it will be conſidered as ſtill more wonderful, if between two Britiſh Obſervatories, Greenwich and Oxford, which have been long ſupplied with great and coſtly inſtruments of the very beſt kinds, there ſhould remain an uncertainty in this reſpect of 2 or 3 ſeconds of time: for in the latitude of Greenwich 3 ſeconds correſpond to 477, and in that of Oxford to 474½ fathoms. Theſe, however, are points which muſt be left to the reſpective aſtronomers to ſettle in the beſt way they can; and it is not to be doubted, that the Aſtronomer Royal will throw a new and very ſatisfactory light on the matter, in the Paper which he propoſes about this time to lay before the Royal Society, along with M. CASSINI's Memoir, which, for that purpoſe, has now been nearly two years in his poſſeſſion.

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With regard to the trigonometrical operation (which may be conſidered as infallible, becauſe, by means of the baſe of verification, it will prove itſelf, and if ſmall errors unavoidably ariſe in the courſe of a long ſuite of triangles the maximum of theſe may be always aſcertained), I have no doubt that the diſtance between Greenwich and the point P in the map may thereby be determined to a very ſmall number of fathoms, perhaps to fifteen or ſixteen on a difference of longitude of about 2° 20′ 20″, and therefore to about $\frac{1}{20}$th part of a ſecond of time on each degree. This, for any uſeful purpoſe, will certainly be admitted to be ſufficiently near the truth, and is probably conſiderably nearer than it will be brought for many years to come, by a mean of the beſt obſervations of the heavenly bodies, if theſe ſhould be found in the preſent ſtate of the matter to leave it yet doubtful to two or three ſeconds.

The aſtronomical difference of time may likewiſe be obtained by experiments on the inſtantaneous exploſion of light; but theſe I would propoſe to be made ſubſequently to the trigonometrical operations. The ſtation of *Tatterlees*, towards the eaſtern extremity of our range of chalk hills, or ſome point near it, would ſeem to be the moſt proper for the place of exploſion, becauſe it can be ſeen from *Bottle-hill*, on the ſame range, and nearly in the meridian of Greenwich Obſervatory. It is not to be doubted, that *Tatterlees* may be ſeen from *Fienne Windmill*, or even perhaps from that of the *Brunemberg*; ſince they are both ſituations, on the continuation of the ſame range in France, the diſtance being ſhorter too, and little land, but chiefly ſea, intervening. Let us then ſuppoſe, that the two aſtronomers with their clocks and tranſit-inſtruments are poſted, one at *Bottle-hill*, and the other at the *Brunemberg*, while gunpowder is repeatedly exploded at *Tatterlees*, or while the Indian lights

lights are alternately exhibited, and again covered by an extinguiſher prepared for the purpoſe, which operation may be repeated ſeveral times the ſame evening; it is certain, that a juſt mean being taken between the inſtants ſo marked by the reſpective clocks, well regulated before-hand, the difference of time between the two extreme ſtations will thereby be obtained to a very conſiderable degree of accuracy, and probably more to be relied upon than that reſulting from the compariſon of the obſervations of the heavenly bodies.

Rockets have been propoſed by ſome to be applied in this way; but even the largeſt ſort, carrying up too ſmall a body of white light, could, I think, only be depended upon for ſhort diſtances; and ſince the actual error of obſervation would be the ſame on a ſhort as on a long diſtance, remote ſtations only ſhould be choſen for concluſive experiments of this nature. It is imagined, that by means of a balloon, a ſet of light-balls might be ſent up of ſuch a volume as to be ſeen, from their great elevation in the air, at ſtations very remote from the place of exploſion. The balloon would aſcend with a burning fuze attached to the firſt ball, and having attained the wiſhed-for height, it might be made faſt below, by means of a cord fixed to it for the purpoſe *. The firſt ball, on its exploſion, would communicate fire to the ſecond, and ſo in ſucceſſion, at intervals of time proportionable to the lengths of

* It is to be underſtood, that the practicability of faſtening the balloon to the earth by means of a rope muſt be previouſly aſcertained: for if that ſhould be found impoſſible, without the wind forcing it down again to the ground, then it muſt be ſent up detached with burning fuzes fixed to it, of ſtill greater dimenſions. By means of theſe it is imagined, that the track of the balloon in the air might be pointed out to the remote obſervers, who might perhaps be better enabled thereby to watch for and diſtinguiſh the ſucceſſive inſtants of the burſting of the white lights.

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the fuzes. Thus 8 or 10 inſtants might be marked from the experiment of a ſingle balloon, which might then be hauled down to be reloaded for a repetition. But whatever might be the mode adopted as the beſt for conducting experiments of this nature, the obſervers muſt not only be very attentive and diligent, but alſo quick-ſighted, have their clocks nicely regulated indeed, and the trials muſt be many times repeated before the uncertainty, even in this way, which ſeems to be the beſt mode, could be reduced to leſs than $\frac{1}{10}$th part of a ſecond of time, to which it may infallibly be brought by trigonometry.

Having in this manner ſhewn what probable degree of exactneſs may be expected in the various, but uſual, ways of aſcertaining the difference of longitude between the Obſervatories of Greenwich and Paris, and compared the reſults with the uncertainty that ſeems yet to exiſt in this matter from the ſtate of aſtronomical obſervations; let us next ſee how Mr. RAMSDEN's inſtrument is likely to perform, when actually applied to the determination in queſtion, by the obſerved angle between the pole ſtar in its eaſtern or weſtern azimuth, and a very remote ſtation, whoſe diſtance from the inſtrument is known by the ſeries of triangles, and diſtinguiſhable by the Indian lights at night, for the purpoſe of this particular obſervation.

With an inſtrument, carrying teleſcopes ſo good that the pole ſtar may be ſeen in daylight, it is obvious, that the biſected angle between the ſtar in its eaſtern and weſtern azimuths will give at once the polar diſtance of the ſtar, and the true meridian of the place, as referred to any known ſtations viſible at the time of obſervation. But as cloudy weather may often prevent a complete obſervation of this ſort from being obtained, and ſince much time might be loſt in

attempting it, therefore the declination of the star settled for any particular period being accurately known*, its apparent distance from the pole may, by the established rules, be readily computed for any proposed day, as well as the precise times of its greatest elongations, twice in 24 hours, when in its eastern and western azimuths, at which times it will, for several minutes, appear, as to sense, stationary or without motion, except in altitude. These are, therefore, the best times for taking the angle between the star and any particular station, since the observations may be repeated frequently in the space of a few minutes, or until it shall be perceived that the star has again approached towards the pole. Now, suppose the station of the instrument to be at *Tatterlees*, whose distance from the perpendicular to the meridian of Greenwich, and consequently from its parallel, is known by the trigonometrical operation. The latitude of the station becomes known likewise; and let the co-latitude be 38° 54′ 20″. Let us likewise suppose the distance of *Bottle-hill* on one side to be 44100 fathoms, equal to 43′ 28″.6 of a great circle; and that of the *Brunemberg* on the other to be 38250 fathoms, equal to 37′ 42″.6 of a great circle; and further, that on these two stations the Indian lights are exhibited for the time proposed. Now, let the angle between the meridian and *Bottle-hill*, and that between it and the *Brunemberg*, be observed by means of the pole star corrected for its distance for the day; and suppose the first to be 75° 10′,

* Since this Paper was written, the Astronomer Royal has been so obliging as to furnish me with the mean distance of the pole star from the pole, as settled at Greenwich by eight observations above and nine below it, made in the year 1786; whereby it appears, that the mean distance, reduced to the beginning of that year, was 1° 50′ 8″.35; and the mean annual precession in declination being 19″.55, consequently, the mean distance for the 1st of January, 1787, was 1° 49′ 8″.8.

and the laſt 125° 5′; thus we ſhall have two ſpherical triangles to compute, in each of which two ſides, and the contained angle, are known, and one ſide, *viz.* the co-latitude, is common to both. Now, from theſe *data*, making uſe of the half ſum and half difference of the ſides, we ſhall have the angles in theſe two triangles as * underneath, and the angle of longitude between *Bottle-hill* and the *Brunemberg*, equal to that at the pole, will be found to be 1° 55′ 56″.12. If from this angle we deduct about 30″ or 35″, for the ſpace that the *Bottle-hill* ſeems to be to the weſtward of the meridian of Greenwich, there will then remain 1° 55′ 21″ for the eaſt longitude of the *Brunemberg*, being very nearly that expreſſed in the map of the triangles which accompanies this paper.

* Bottle-hill.	°	′	″
Half difference –	51	25	14.05
Half ſum –	52	32	25.07
Angle at Bottle-hill	103	57	39.12
Angle at the pole –	1	7	11.02
Contained angle –	75	10	0.
Sum of the three angles	180	14	50.14
Angle of convergence		52	20.88
Exceſs above 180° –		14	50.14
Angle of longitude –	1	7	11.02

Brunemberg.	°	′	″
Half difference –	26	44	12.2
Half ſum –	27	32	57.3
Angle at Brunemberg	54	17	9.5
Angle at the pole –		48	45.1
Contained angle –	125	5	0.
Sum of the three angles	180	10	54.6
Angle of convergence –		37	50.5
Exceſs above 180° –		10	54.6
Angle of longitude –		48	45.1

Sum of the two longitudes 1° 55′ 56″.12.

It is to be obſerved, that the meridians, which are all parallel to each other at the equator, on their departure from thence converge more and more as they approach towards the pole, where the angle of convergence becomes equal to the angle of longitude. It may alſo be remarked, that the angle of convergence, augmented by the exceſs of the three angles of the ſpherical triangle above 180°, is always equal to the angle of longitude, or that at the pole. And as this holds univerſally in all latitudes, it affords a ready means of proving that the computations are juſt.

As far as we are enabled to judge at present, from the examination of the divisions of Mr. RAMSDEN's instrument, there is every reason to believe, that in taking angles around the horizon, the mean of several repetitions of the same angle, as referred to different parts of the circumference of the circle, will differ very little from the truth, so little indeed, that in many cases the error will totally vanish. But in elevating the telescope towards the pole, let us suppose that an error of 5 seconds on each of the contained angles at *Totterlees* has been committed; and further, that even an error of 5 seconds of latitude, equal to about 84½ fathoms on the meridian, may have been fallen into, in estimating the co-latitude (which never can happen, but is only here admitted, to place the example in the most disadvantageous circumstances possible); then whoever will give themselves the trouble to recompute the two triangles with these new *data*, will find the result in longitude not to be varied thereby, in the first case above $\frac{1}{10}$th part of a second, or $\frac{1}{15}$th part of a second in time; and in the last not quite 1 second, or $\frac{1}{15}$th part of a second in time. Hence I conclude, that the best mode of determining the differences of longitude will be by the instrument itself, applied in this way, in taking the angles between the pole star and very remote stations, distinguishable at night by the help of the Indian lights, and whose distance is accurately known. This method will, it is true, be liable, as well as astronomical observations, to the imperfections of the instrument, particularly those of the telescope, and the unavoidable error in its application; but, on the other hand, it will be entirely free from the irregularities of clocks, and the imperfections of vision in marking the instantaneous explosion of light. When both methods have been repeated a sufficient number of times, with all

imaginable care, we ſhall then, and not till then, be able to judge to which the preference may be due. Thus five or ſix long ſtations, in or nearly in the parallel of Greenwich, ſuch, for inſtance, as that of Shooter's-hill Tower, would reach from the eaſt quite to the weſt of the iſland: and as a very conſiderable degree of conſiſtency might be expected among the reſults for equal portions of the parallel, this method ſeems to be as likely as any to furniſh *data* for determining the nature of the ſpheroid or figure of the earth.

Table of the degrees of the earth, conſtructed on the hypotheſis of M. Bouguer.

Beſide the table of compariſon of the arc between Greenwich and Perpignan, which I have already endeavoured to explain, this Paper is accompanied with another, which, as well as the former, was originally intended ſolely for my own uſe. With this view it was at firſt only computed for every five minutes of the 51ſt and 52d degrees of latitude, that I might thereby be enabled more readily to compare the longitudes and latitudes of the reſpective ſtations in our progreſs towards the coaſt; and more particularly to fit it for operations in the ſouth parts of England, as likely to be firſt carried into execution. In order, however, to render it more generally uſeful, it has ſince been extended to every five degrees in the higher and lower parts of the quadrant, and to every ſingle degree in intermediate latitudes. The table not only contains the degrees of the meridian and of longitude, but alſo thoſe of a great circle perpendicular to the meridian, and likewiſe ſuch as are oblique to it, for the other ſeven points of the compaſs. With regard to the conſtruction of the table, it is only neceſſary to make

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ſome remarks on that column of it, which contains the ſum of the three equations, or difference between the degree of the meridian and the correſponding degree of the great circle perpendicular to it, which is the moſt troubleſome to compute, but muſt be found before the degree of longitude can be obtained. The firſt part of M. BOUGUER's equation conſiſts of $\frac{1}{7}$ths of the difference between the degree of the meridian at the equator and that at the pole, *viz.* 545.12 fathoms, to be conſtantly added. Secondly, $\frac{1}{3}$ths of the increment of the correſponding degree of the meridian above that at the equator, to be ſubtracted; and, thirdly, $\frac{4}{7}$ths of a third proportional to the exceſs of the degree at the pole above that at the equator as radius, and the ſine of the correſponding latitude, to be added. Now it will be found, that this laſt part of the equation $\frac{4}{7}$ths, if uniformly applied, would have produced abſurd reſults at the 75th degree of latitude, that is to ſay, the degrees of a great circle would there have become greater than the degree at the pole. The equation $\frac{4}{5}$ths or $\frac{1}{1}$th would in like manner have produced abſurd reſults between the 79th and 80th degree. Even $\frac{1}{3}$ths will not go on further than the 85th; and the higheſt equation that will go through the whole quadrant, uniformly applied, muſt not exceed $\frac{1}{170}$ parts of the third proportional. M. BOUGUER himſelf had found this, and accordingly had applied the equation with a certain modification or abatement, which nevertheleſs he makes no mention of in his book. Seeing, therefore, that the degrees of a great circle perpendicular to the meridian differ moſt from thoſe of latitude about the tropics, I have, at the 20th degree, applied the equation $\frac{4}{7}$ths or $\frac{1}{300}$ parts, and made the diviſor increaſe by unity for each degree of the quadrant above that point to the pole, where it becomes $\frac{1}{170}$ parts. Below the 20th degree

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the divisor, in like manner, increases by unity to the equator, where it becomes $\frac{1}{110}$ parts. With respect to the degrees of great circles, obliquely situated to the meridian, they were chiefly intended for facilitating the computations of curvature and refraction, where, in any particular case, it might become necessary to obtain, to great precision, the relative height of a very distant station with regard to that occupied by the instrument: for in such cases, in strictness, the angle which the line makes with the meridian should be attended to.

The computations * for the construction of this table have been, as may easily be judged, sufficiently laborious; but having once begun, I was induced to go through with them, conceiving that a table of this sort would be of general utility; and that it might be very long indeed before the results of future operations, yet to be undertaken in different and very remote parts of the globe, would furnish *data* for any thing better. It must be by the united efforts of enlightened nations, conspiring as it were together in promoting the cause of science, that great objects, such as the determination of the magnitude and figure of the earth, can ultimately be obtained. Each should contribute to it, and all have it more or less in their power, according to their particular situation, and that

* Much care has been bestowed in the various computations for the construction of the two tables appertaining to this Paper. In the last, which has been the most laborious, the differences seem to go on so uniformly, it is hoped, no error of any consequence will be found. Here it is proper that I should mention a typical *erratum* in one of the tables belonging to my Paper on the Barometer, published in the LXVII. volume of Transactions, for 1777. It is in Tab. VI. containing observations on heights near Carnarvon. In the column of observed height of Mercury on Moel Eilio, August 4, 1775, 1 h. 7 m. P. M. instead of 27.714 read 27.214.

portion

portion of its ſurface which has fallen to their ſhare in the general diſtribution of things.

Under the auſpices, and immediately under the eye, of a Sovereign who loves and cheriſhes the ſciences, much may be done within the limits of the Britiſh iſlands, which, reckoning from Eaſtneſs in Suffolk to the weſtern parts of Kerry in Ireland, contain near 12 degrees of longitude; and counting from the Channel on the ſouth to the Orkney iſlands only on the north about 9 degrees of latitude. With this general purpoſe in view, and at the ſame time to lay the beſt foundation for the ſurvey of the Britiſh iſlands, I would propoſe, as the operations preferably to be executed, that ſerieſes of triangles ſhould be extended along different meridians, particularly thoſe of the chief Obſervatories, and of ſome remarkable hills in the central parts of the iſland, till they fall into the ſea on the ſouth and north; and that, acroſs theſe, a great number of ſerieſes, in the parallels likewiſe of the chief Obſervatories, and of remarkable hills or eminencies, ſhould be extended from the eaſt to the weſt coaſt. The *Wrekin-hill*, on the eaſtern ſkirt of Shropſhire, ſeems to be an eligible point, being in a central ſituation, ſufficiently conſpicuous, and yet not ſo high as to render the frequent acceſs to it very inconvenient. If it ſhould be found, that longitudes may be determined by the inſtrument to the degree of accuracy which is expected, and that it may likewiſe be advantageouſly applied, independently of the zenith ſector, in tracing any parallel whoſe latitude has been already aſcertained, or the parallel of any remarkable point, whoſe meridian is known, but not its latitude; then the number of theſe *parallel ſerieſes* cannot be too much multiplied: for a great many of theſe operations executed with due care would

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abſolutely give the figure of the earth, ſince the ſhorteſt diſtance from the ſurface to its axis would thereby be obtained.

The Britiſh dominions in the Eaſt-Indies offer a ſcene particularly favourable for the meaſurement of five degrees of latitude on the coaſt of Choromandel, as has been noticed by Mr. DALRYMPLE, F. R. S. in his Paper on the Marine Survey of that Coaſt. Two degrees of longitude, at each extremity of this arc, ſhould likewiſe be meaſured.

The plains of Bengal, directly under the northern tropic, afford another ſituation where it would be of great conſequence to determine the lengths of a degree or two of latitude, and as many of longitude. Theſe two operations could not fail to be patroniſed by the Eaſt-India Company, who ſhould defray the expence; ſince, whatever tended ſo much to the improvement of ſcience in general, and ſo directly to that of navigation in particular, muſt be thought important to a Body of Merchants, whoſe power, as well as opulence, ſtand at this day unequalled in the mercantile hiſtory of the world.

But there is one operation yet to be mentioned that would contribute more than any other to the determination of the figure of the earth, which is, the accurate meaſurement of ſome degrees of longitude on the equator; becauſe thereby the length of its ſemi-diameter would be immediately known, which hitherto has only been theoretically computed from the meaſured portions of the meridian. The Portugueſe ſeem to be poſſeſſed of the moſt advantageous ſituation yet known on the globe for that purpoſe: for M. DE LA CONDAMINE has told us, that at the mouth of the river of Amazons, near the fort of *Macapa*, in three minutes north latitude, there are

extenſive open plains where an operation of this ſort would not have been ſo difficult as was at firſt imagined, when he propoſed the ſcheme to the Academy of Sciences, a year before the voyage to *Quito* was thought of. Had this been adopted as the ſcene of their operations, inſtead of that elevated valley comprehended between the lofty ranges of the *Andes*, the buſineſs would have been much ſooner accompliſhed, with infinitely leſs labour and fatigue, and ſtill more ſatisfactorily, ſince degrees of longitude as well as of latitude would probably have been meaſured. Here then the Portugueſe ſhould determine the length of two or more degrees of longitude, and as many of latitude, by way of proof or correction of the Peruvian obſervations. A well conducted operation of this kind could not fail to add to the celebrity of a nation who, by their diſcovery of the new world in the weſt, and having opened the route by the Cape of Good Hope to the eaſtern ſide of the globe, may be conſidered as the founders of modern navigation.

With regard to operations in high northern latitudes, which would doubtleſs be of great importance, the Ruſſian Empire muſt certainly afford a variety of ſituations, more or leſs difficult, for the purpoſe; and an Empreſs who commands ſo great a proportion of that region of the world, where it is ſaid exploratory diſcoveries are at preſent carrying on by her order, can ſcarcely be ſuppoſed to ſuffer a reign, otherwiſe ſo brilliant, to expire, without directing ſomething of this ſort to be executed under the polar circle, or as near to the pole as the ſeverity of the climate will permit, by way of confirmation or correction of the Lapland meaſurement, which yet ſtands ſingle and by itſelf, without any collateral proof of its exactneſs. If ſuch operations as thoſe I have ſuggeſted towards the

close of this Paper were well executed in different parts of the world, little would then remain to be done towards the determination of the magnitude and figure of the earth, except multiplying, as much as possible, experiments with the same pendulums from the equator to very high southern and northern latitudes, that some judgement might thereby be formed of the similarity, or otherwise, of the two polar sides of the spheroid.

WILL. ROY.

Feb. 22, 1787.

ERRATA.

In the Second Table subjoined to this Paper, page 228, under the head "Degrees of Longitude," in the column of the length of each degree in fathoms, l. 24. from the bottom (being that which answers to the Latitude of Greenwich) for 38161.69 read 38164.00; and in the next column of differences, l. 25. from the bottom, for 53.13 read 50.82; and the next line below, for 16.12 read 18 43.

In the Map of the Triangles, for SELSEY read SELSEY.

Comparison

Comparison of the observed length of the celestial arc of the meridian, comprehended between the parallels of Greenwich

CELESTIAL ARC

Comprehended between the parallels of Greenwich and Perpignan.

TERRES

Computed

	Places of Observation.	Latitudes. ° ′ ″ ‴	Differences of latitudes or celestial arcs. Deg. ° Min. ′ &c. ″ ‴	Deg. °	Dec. parts	Measured Arc. Fathoms.	Supposing the earth to be a sphere. Fathoms.	Error.	Mean error on each deg. +73.09.	Supposing the 1 Fathoms.	Error.	2 Fathoms.	Error.	3 Fathoms.	Er
1	Greenwich Roy. Obsev.	51 28 40 00	} 8 46 48 00	8.	78		534343.			535927.		535588.		535088.	
	Perpignan –	42 41 52 00					27235.			27350.		27331.		27301.	
	Greenwich and M=	MP	diff. 0 26 50 52	0.	4475										
2	Point M near Dunkirk	51 1 49 8	} 8 19 57 8	8.	3325	506499.	507108.	+ 609		508577.	+ 2078	508257.	+ 1758	507787.	+ 12
	Perpignan –	42 41 52 00					133467.	+ 50		133934.	+ 517	134038.	+ 621	133747.	+
	Point M and Paris	— — — —	diff. 2 11 35 8	2.	1931	133417.									
3	Paris Royal Observatory	48 50 14 00	} 6 8 22 00	6.	1394	373082.	373641.	+ 559		374603.	+ 1521	374374.	+ 1292	374040.	+
	Perpignan –	42 41 52 00					106658.	+ 94		107007.	+ 443	106937.	+ 373	106834.	+
	Paris and Bourges	— — — —	diff. 1 45 9 19	1.	7525	106564.									
4	Bourges –	47 5 4 41	} 4 23 12 41	4	3869	266518.	266983.	+ 465		267596.	+ 1078	267437.	+ 918	267206.	+
	Perpignan –	42 41 52 00					166200.	+ 191		166620.	+ 420	166518.	+ 509	166370.	+
	Bourges and Rodés	— — — —	diff. 2 43 51 5	2.	6309	166009.									
5	Rodés –	44 21 13 36	} 1 39 21 36	1	656	100509.	100783.	+ 274		100976.	+ 467	100919.	+ 410	100836.	+

+ 249.38. + 210.98.

	Semi-diameter of the equator – –						3486975	} .00000		3504545	} 19572	3501802	} .18194	3497814	} 1
	Semi-axis between the poles – –						3486975			3484973		3483608		3481624	2
	Ratio of the semi-diameters – –									179.047 to	178 047	192.483 to	191.483	216.06 to	
	Difference of the logarithms of the ratio –									0.0024323		0.0022622		0.0020148	
Some other chief properties.	Degrees of the meridian at the	Equator – –				60484.5	60859.1	Error. + 374.6		60484.5	Error. 0.0	60484.5	Error. 0 0	60484.5	E
		45th – –				60777.6	60859.1	+ 81.5		60993.4	+ 215.8	60957.7	+ 180.1	60905.9	+ 1
		66° 20′ – –				61194.3	60859.1	− 335.2		61342.3	+ 148.0	61281.9	+ 87.6	61194.3	
		Pole – –					60859.1			61509.3		61437.1		61332.2	
	Excess of the degree of the meridian at the pole, above that at the equator – –						00000.0			1024.8		952.6		847.7	
	Degrees of longitude at the	Equator – –					60859.1			61165.8		61117.9		61048.5	
		43° 32′ south of France –				44354.4	44121.3	− 233.1		44461.2	+ 106.8	44430.4	+ 76.0	44355.7	+
		51° 28′ 40″ Greenwich –					37904.2			38225 9		38181.3		38130.1	

nwich and Perpignan, with the measured and computed lengths of the corresponding terrestrial arc between these parallels.

S T R I A L A R C.

puted arc on different *hypotheses*.

ng the Earth to be an oblate ellipsoid.

Supposing the earth to be a spheroid differing from the ellipsoid.

More prominent in middle latitudes than the ellipsoid.

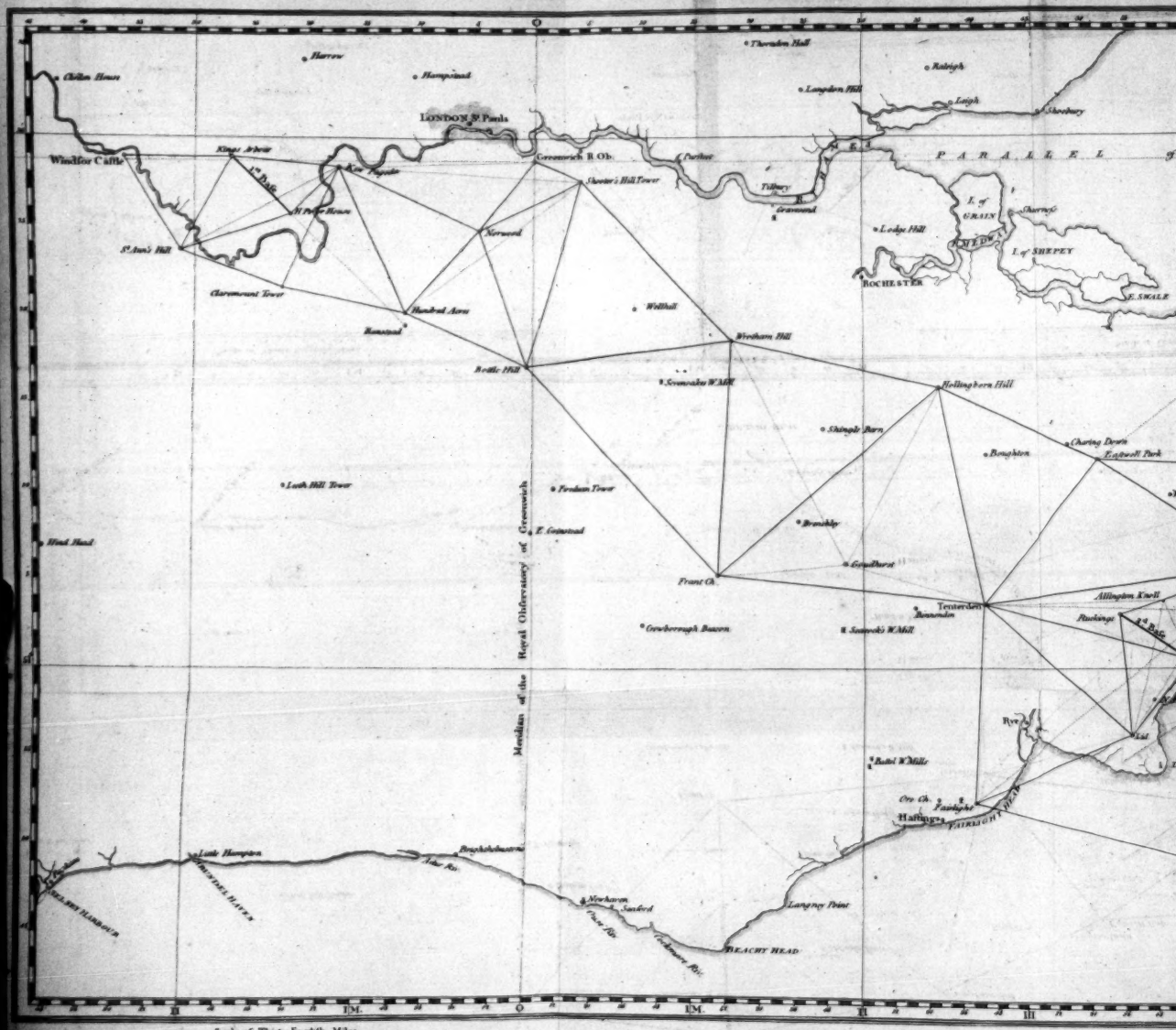
Less prominent in middle latitudes than the ellipsoid of the same semi-diameters.

3		4		5		6		7		1		2	
	Error.	Fathoms.	Error.	Fathoms.	Error.	Fathoms.	Error.	Fathoms.	Error.	Fathoms.	Error.	Fathoms.	Error.
		534970.		533942.		533860.		533751.		534997.		533737.	
		27294.		27241.		27230.		27206.		27296.		27243.	
	+1288	507676.	+1177	506701.	+ 202	506630.	+ 131	506545.	+ 46	507701.	+1202	506494.	− 5
	+ 330	133715.	+ 298	133454.	+ 37	133412.	− 5	133360.	− 57	133722.	+ 305	133438.	+21
	+ 958	373961.	+ 879	373247.	+ 155	373218.	+ 136	373185.	+ 103	373979.	+ 897	373056.	−26
	+ 270	106810.	+ 246	106605.	+ 41	106582.	+ 18	106555.	− 9	106815.	+ 251	106570.	+ 6
	+ 688	267151.	+ 633	266642.	+ 124	266636.	+ 118	266630.	+ 112	267164.	+ 646	266486.	−32
	+ 361	166334.	+ 325	166017.	+ 8	166005.	− 4	165991.	− 18	166342.	+ 333	165928.	−81
	+ 327	100817.	+ 308	100625.	+ 116	100631.	+ 122	100639.	+ 130	100822.	+ 313	100558.	+49
	+154.58.		+141.25.		+24.24.		+15.73.		+5.52.		+14.43.		−0.6.
	16190	3496934	15713	3489949	15174	3487954	11236	3485542	6455	3496934	15713	3496740	19530
to	215.06	3481221		3474775		3476718		3479087		3481221		3477210	
48		222.55 to	221.55	230. to 229		310.3 to	309.3	540 to 539		222.55 to	221.55	179.4 to	178.4
		0.0019558		0.0018924		0.0014018		0.0008050		0.0019558		0.0024274	
	Error.		Error.		Error.		Error.		Error.		Error.		Error.
5	0.0	60484.5		60382.6	− 101.9	60484.5	0.0	60609.0	+ 124.5	60484.5	0.0	60484.5	0.0
9	+128.3	60893.5	+115.9	60777.6	0.0	60777.6	0.0	60777.6	0.0	60896.5	+ 118.9	60740.0	−37.6
3	0.0	61173.4	− 20.9	61047.8	− 146.5	60977.7	− 216.6	60891.6	+ 302.7	61175.7	− 18.6	61203.7	+ 9.4
2		61308.4		61177.0		61073.0		60947.0		61308.5		61506.6	
7		823.9		794.4		588.5		338.0		824.0		1022.1	
5		61033.0		60911.1		60876.2		60834.1		61033.0		61029.6	
7	+ 1.3	44341.7	− 12.7	44250.1	− 104.3	44213.4	− 141.0	44141.9	− 212.5	44341.8	− 12.6	44373.5	+19.1
1		38117.7		37978.7		37989.9		37932.4		38117.2		38161.7	

h

Table

MAP *shewing nearly the* DISPOSITION *of the* TRIANGLES, *whereby it is proposed to*



...d to determining the Relative Situations of the Royal Observatories of GREENWICH and PARIS.

Philos. Trans. Vol. LXXVII. Tab. IX. p. 218.

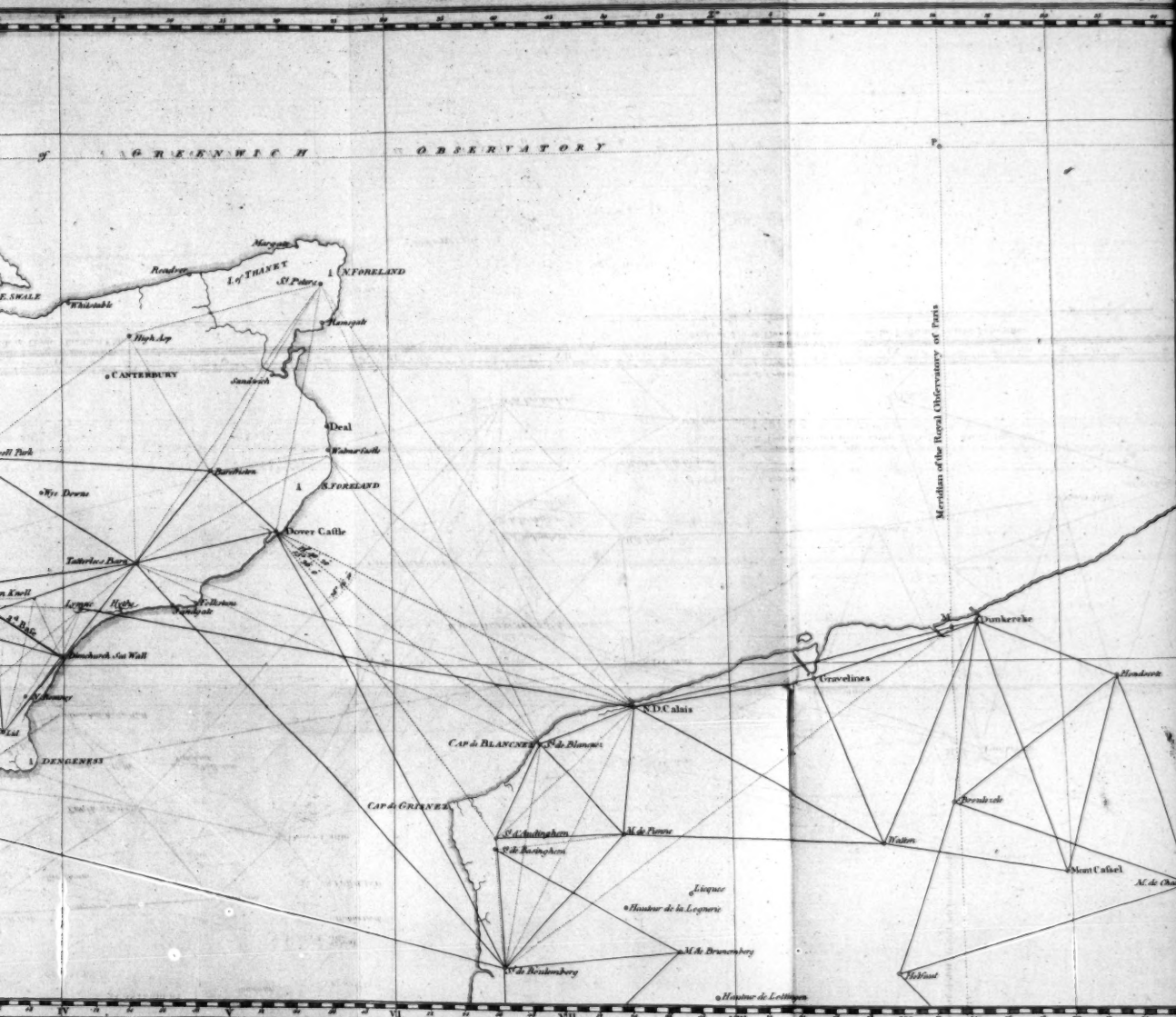


Table of the degrees of the Earth constructed on the hypothesis of M. Bouguer, where the increments to
squares of the sines of t

Latitudes.	Lengths of degrees of the meridian in fathoms.	Diff.	Increments above the deg. at the equator.	Diff. between deg. of the mer. and deg. of great circles perp. to the mer. or sum of the three equations.	Degrees of great circles perpendicular to the meridian.	Diff.
° ′ ″						
0 0 0	60484.50	0.06	0.00	545.12	61029.62	1.95
5 — —	60484.56	0.87	0.06	547.01	61031.57	5.63
10 — —	60485.43	3.65	0.93	551.77	61037.20	11.52
15 — —	60489.08	9.41	4.58	559.64	61048.72	16.28
20 — —	60498.49	18.62	13.99	566.51	61065.00	20.65
25 — —	60517.11	31.27	32.61	568.54	61085.65	25.78
30 — —	60548.38	46.75	63.88	563.15	61111.53	31.14
35 — —	60595.13	63.86	110.63	547.54	61142.67	36.19
40 — —	60658.99		174.49	519.87	61178.86	
		14.86				7.73
41 — —	60673.85		189.35	512.74	61186.59	
42 — —	60689.40	15.55	204.90	505.14	61194.54	7.95
43 — —	60705.62	16.22	221.12	497.02	61202.64	8.10
44 — —	60722.50	16.88	238.00	488.40	61210.90	8.26
45 — —	60740.01	17.52	255.52	479.28	61219.30	8.40
46 — —	60758.17	18.15	273.67	469.64	61227.81	8.51
47 — —	60776.92	18.75	292.42	459.55	61236.47	8.66
48 — —	60796.24	19.32	311.74	449.00	61245.24	8.77
Paris 48 50 14	60812.83	16.59	328.33	439.77	61252.60	7.36
49 — —	60816.10	3.37	331.60	437.93	61254.03	1.43
50 — —	60836.47	20.37	351.97	426.52	61262.99	8.96
		1.72				0.76
50 5 —	60838.19	1.73	353.69	425.56	61263.75	0.74
50 10 —	60839.92	1.72	355.42	424.57	61264.49	0.74
50 15 —	60841.64	1.73	357.14	423.59	61265.23	0.78
50 20 —	60843.37	1.74	358.87	422.64	61266.01	0.76
50 25 —	60845.11	1.73	360.61	421.66	61266.77	0.74
50 30 —	60846.84	1.74	362.34	420.67	61267.51	0.75
50 35 —	60848.58	1.74	364.08	419.68	61268.26	0.74
50 40 —	60850.32	1.75	365.82	418.68	61269.00	0.79
50 45 —	60852.07	1.75	367.57	417.72	61269.79	0.74
50 50 —	60853.82	1.75	369.32	416.71	61270.53	0.74
50 55 —	60855.57	1.75	371.07	415.70	61271.27	0.75
51 0 —	60857.32		372.82	414.70	61272.02	
		1.76				0.74
51 5 —	60859.08	1.76	374.58	413.68	61272.76	0.77
51 10 —	60860 84	1.77	376.34	412.69	61273.53	0.78
51 15 —	60862.61	1.76	378.11	411.70	61274.31	0.74
51 20 —	60864.37	1.77	379.87	410.68	61275.05	0.75
51 25 —	60866.14	1 30	381.64	409.66	61275.80	0.54
Greenwich 51 28 40	60867.44	0.48	382.94	408.90	61276.34	0.21
51 30 —	60867.92	1.77	383.42	408.63	61276 55	0.75
51 35 —	60869.69	1.78	385.19	407.61	61277.30	0.74
51 40 —	60871.47	1.78	386.97	406.57	61278.04	0.80
51 45 —	60873.25	1.79	388.75	405.59	61278.84	0.74
51 50 —	60875.04	1.78	390.54	404.54	61279.58	0.75
51 55 —	60876.82	1.79	392.32	403.51	61280.33	0.76
52 0 0	60878.61		394.11	402.48	61281.09	
		21.69				9.09
53 — —	60900.30	22.05	415.80	389.88	61290.18	9.13
54 — —	60922.35	22.35	437.85	376.96	61299.31	9.14
55 — —	60944.70	22.63	460.20	363.75	61308.45	9.14
56 — —	60967.33	22.83	482.83	350.26	61317.59	9.10
57 — —	60990.16	23.00	505.66	336.53	61326.69	9.07
58 — —	61013.16	16.46	528.66	322.60	61335.76	6.45
Equal to deg. of Inog. on the equator. 58 42 47.4	61029.62	6.65	545.12	312.59	61342.21	2.57
59 — 0	61036.27	23.16	551.77	308.51	61344.78	8.94
60 — 0	61059.43		574.93	294.29	61353.72	
		114.67				42.98
65 — —	61174.10	29.65	689.60	222.60	61396.70	10.80
Deg. meas. in Lapland. 66 20 7	61203.75	77.71	719.25	203.75	61407.50	27.89
70 — —	61281.46	92.79	796.96	153.70	61435.16	31.80
75 — —	61374.25	71.64	889.75	92.71	61466.96	24.31
80 — —	61445.89	45.24	961.39	45.38	61491.27	12 33
85 — —	61491.13	15.47	1006.63	12.47	61503.60	3.00
90 — —	61506.60		1022.10	0.00	61506.60	

Degrees of great circles oblique to the mer
perpendicular to them, bein

Diff.	7 points = 78° 45′.	Diff.	6 points = 67° 30′.	Diff.	5
26.75	61008.87	59.08	60949.79	88.43	60
20.82	61010.75	59.29	60951.46	88.73	60
21.00	61016.20	59.80	60956.40	89.51	60
21.30	61027.42	60.66	60966.76	90 87	60
21.56	61043.44	61.40	60982.04	91.90	60
21.64	61064.01	61.62	61002.39	92.24	60
21.43	61090.10	61.04	61029.06	91.35	60
20.86	61121.83	59.35	61062.48	88.81	60
19.78	61159.08	56.35	61102.73	84.33	60
19.51	61167.08	55.58	61111.50	83.17	60
19.23	61175.31	54.75	61120.56	81.94	60
18.92	61183.72	53.87	61129.85	80.61	60
18.59	61192.31	52.93	61139.38	79.23	60
18.24	61201.06	51.95	61149.11	77.74	60
17.86	61209.95	50 c0	61159.05	76.18	60
17 49	61218.98	49 81	61169.17	74.55	60
17.09	61228.15	48.66	61179 49	72.84	60
16.74	61235.86	47.66	61188.20	71.34	60
16.67	61237.36	47.45	61189.90	71.04	60
16.23	61240.76	46.23	61200.53	69 19	60
16.20	61247.55	46.12	61201.43	69.03	60
16.16	61248.33	46.02	61202.31	68.87	60
16.12	61249.11	45.91	61203.20	68.71	60
16.09	61249.92	45.80	61204.12	68.56	60
16.05	61250.72	45.70	61205.02	68.40	60
16.01	61251.50	45.60	61205.90	68.23	60
15.97	61252.29	45.49	61206.80	68.08	60
15.93	61253.07	45.38	61207.69	67.92	60
15.90	61253 89	45.27	61208.62	67.76	60
15.86	61254.67	45.17	61209.50	67.59	60
15.82	61255.45	45.06	61210.39	67.43	60
15.78	61256.24	44.95	61211.29	67.27	60
15.74	61257.02	44.84	61212.18	67.11	60
15.71	61257.82	44.73	61213.09	66.94	60
15.67	61258.64	44.62	61214.02	66.79	60
15.63	61259.42	44.51	61214.91	66.62	60
15.59	61260.21	44.40	61215.81	66.45	60
15.56	61260.78	44.32	61216.46	66.33	60
15.55	61261.00	44.29	61216.71	66.29	60
15.51	61261.79	44.18	61217.61	66.12	60
15.47	61262.57	44.07	61218.50	65.95	60
15.44	61263.40	43.95	61219.45	65.80	60
15.40	61264.18	43.84	61220.34	65.62	60
15.36	61264.97	43.73	61221.24	65.46	60
15.32	61265.77	43.02	61222.15	65.29	60
14.84	61275.34	42.26	61233.08	63.24	60
14.35	61284.96	40.86	61244.10	61.14	60
13.84	61294.61	39.43	61255.18	59.00	60
13.33	61304.26	37.96	61266.30	56.82	60
12.81	61313.88	36.47	61277.41	54.59	60
12.28	61323.48	34.96	61288.52	52.33	60
11.90	61330.31	33.88	61296.43	50.70	60
11.74	61333.04	33.44	61299.60	50.05	60
11.20	61342.52	31.90	61310.62	47.74	60
8.47	61388.23	24.13	61364.10	36.11	60
7.75	61399.75	22.09	61377.66	33.05	60
5.85	61419.31	16.66	61412.65	24.93	60
3.53	61463.43	10.05	61453.38	15.04	60
1.73	61489.54	4.92	61484.62	7 36	60
0.47	61503.13	1.36	61501.77	2.02	60
0.00	61506.60	0.00	61506.60	0.00	60

nts to the degrees of the meridian above that at the equator follow the ratio of the 4th power, or squared
s of the latitudes.

he meridian, the differences between the meridional degrees, and those of great circles
n, being in the ratio of the squares of the fines of obliquity.

Diff.	5 points =56°¼.	Diff.	4 points =45°	Diff.	3 points =33°¾.	Diff.	2 points =22°½.	Diff.	1 point =11°¼.	Degrees of longitude.	Diff.	Latitudes.		
												°	′	″
8.43	60861.36	104.20	60757.16	104.40	60652.76	88.43	60564.33	59.08	60505.25	61029.62	230.28	0	0	0
8.73	60862.73	104.66	60758.07	104.67	60653.40	88.73	60564.67	59.22	60505.45	60799.34	689.48	5	–	–
9.51	60866.89	105.57	60761.32	105.58	60655.74	89.51	60566.23	59.80	60506.43	60109.86	1141.33	10	–	–
0 87	60875.89	107.03	60768.26	106.44	60661.82	90.78	60571.04	60.66	60510.38	58968.53	1586.20	15	–	–
1.90	60890 14	108 39	60781.75	108.40	60673 35	91.90	60581.45	61.40	60520.05	57382 33	2019.94	20	–	–
2.24	60910.15	108.77	60801.38	108.79	60692.59	92.22	60600.37	61.62	60538.75	55362.39	2438.26	25	–	–
1.35	60935.71	107.75	60829.96	107.76	60722.20	91.35	60630.85	61 04	60569.81	52924.13	2839.00	30	–	–
8.81	60973.67	104.77	60868.90	104.77	60764.13	88.81	60675.32	59.35	60615.97	50085.13	3219.39	35	–	–
4.33	61018.40	99.48	60918.92	99 47	60819.45	84.33	60735.12	56. 34	60678.78	46865.74	687.62	40	–	–
3.17	61028.33	98.11	60930.22	98.11	60832.11	83.17	60748.94	56.58	60693.36	46178.12	701.72	41	–	–
1.94	61038.62	96.65	60941.97	96.65	60845.32	81.94	60763.38	54.75	60708.63	45476.40	715.63	42	–	–
0.61	61049.24	95.11	60954.13	95.10	60859.03	80.62	60778.41	53.87	60724.54	44760.77	729.34	43	–	–
9.23	61060.15	93.45	60966.70	93.45	60873.25	79.23	60794.02	52.93	60741.09	44031.43	742.85	44	–	–
7.74	61071.37	91.71	60979 66	91.71	60887.95	77.74	60810.21	51.95	60758.26	43288.58	756.17	45	–	–
6.18	61082.87	89.87	60993.00	89.86	60903.14	76.19	60826.95	50 91	60776.04	42532.41	769.24	46	–	–
4.55	61094 62	87.93	61006.69	87.93	60918.76	74.54	60844.22	49.81	60794.41	41763.17	782.11	47	–	–
2.84	61106.65	85.91	61020.74	85.91	60934.83	72 84	60861.99	48.66	60813.33	40981.06	664.55	48	–	–
1.04	61116.10	84.15	61032.31	84.14	60948.57	71.34	60877.23	47.66	60829.57	40316.51	130.25	Paris 48	50	14
1.04	61118.86	83.80	61035.06	83.79	60951.27	71.04	60880.23	47.46	60832.77	40186.26	807.17	49	–	–
0.69	61131.34	81.61	61049.73	81.61	60968.12	69.19	60898.93	46.23	60852.70	39379.09	67.80	50	–	–
69.03	61132 40	81 43	61050.97	81.43	60969.54	69.03	60900 51	46.12	60854.39	39311.29	67.90	50	5	–
68.87	61133.44	81.23	61052 21	81.24	60970.97	68.87	60902.10	46.02	60856.08	39243.39	68.02	50	10	–
68.71	61134 49	81.06	61053.43	81.05	60972.38	68.71	60903.67	45.91	60857.76	39175.37	68.05	50	15	–
68.56	61135 56	80.87	61054.69	80.87	60973.82	68.56	60905.26	45.80	60859.46	39107.32	68.14	50	20	–
68.40	61136.62	80.68	61055 94	80.68	60975.26	68.40	60906.86	45.70	60861.16	39039.18	68.24	50	25	–
68.23	61137.67	80.50	61057.17	80.49	60976.68	68.23	60908.45	45.60	60862.85	38970.94	68.32	50	30	–
68.08	61138.72	80.30	61058.42	80.30	60978.12	68.08	60910.04	45 49	60864.55	38902.62	68.40	50	35	–
67.92	61139.77	80.11	61059.66	80.11	60979.55	67.92	60911.63	45.37	60866.26	38834.22	68.50	50	40	–
67.76	61140.80	79.93	61060.93	79.93	60981.00	67.76	60913.24	45.27	60867.97	38765.72	68.57	50	45	–
67.59	61141.91	79.74	61062.17	79.73	60982.44	67.59	60914.85	45.17	60869.68	38697.15	68.67	50	50	–
67.43	61142.96	79.54	61063.42	79.54	60983.88	67.43	60916.45	45.06	60871.39	38628.48	68.76	50	55	–
67.27	61144.02	79.35	61064.67	79.35	60985.32	67.27	60918.05	44.95	60873.10	38559.72	68.82	51	0	0
67.11	61145.07	79.15	61065.92	79.15	60986.77	67.11	60919.66	44.84	60874.82	38490.90	68.90	51	5	–
66.94	61146.15	78.96	61067.19	78.97	60988.22	66.94	60921.28	44.73	60876.55	38422.00	68.97	51	10	–
66.79	61147.23	78.77	61068.46	78.78	60989.68	66.78	60922.90	44.62	60878.28	38353.03	69.06	51	15	–
66.62	61148.29	78.58	61069.71	78.58	60991.13	66.62	60924.51	44.51	60880.00	38283.97	69.15	51	20	–
66.45	61149.36	78.39	61070.97	78.39	60992.58	66.45	60926.13	44.40	60881.73	38214.82	53.13	51	25	–
66.33	61150.13	78 24	61071.89	78 24	60993.65	66.33	60927.32	44.32	60883.00	38161.69	16.12	Greenwich 51	28	40
66.29	61150 42	78.18	61072.24	78.19	60994.05	66.29	60927.76	44.29	60883.47	38145.57	69.33	51	30	–
66.12	61151.49	78.00	61073.49	77.99	60995.50	66.12	60929 38	44.18	60885.20	38076.24	69.41	51	35	–
65.95	61152.55	77.79	61074.76	77.80	60996.96	65.95	60931.01	44.07	60886.94	38006.83	69.49	51	40	–
65.80	61153 65	77.60	61076.05	77.61	60998.44	65.80	60932.64	43.95	60888.69	37937.34	69.57	51	45	–
65.62	61154.72	77.41	61077.31	77.41	60999.90	65.62	60934.28	43.84	60890.44	37867.77	69.65	51	50	–
65.46	61155.78	77.21	61078.57	77.20	61001.37	65.46	60935.91	43.73	60892.18	37798.12	69.72	51	55	–
65.29	61156.86	77.01	61079.85	77.01	61002.84	65.29	60937.55	43.62	60893.93	37728.40	843.05	52	0	0
63.24	61169.84	74.60	61095.24	74.60	61020.64	63.24	60957.40	42.26	60915.14	36885.35	854.52	53	–	–
61.14	61182.96	72.13	61110.83	72.13	61038.70	61.15	60977.55	40.85	60936.70	36030.83	865.75	54	–	–
59.00	61196.18	69.60	61126.58	69.61	61056.97	59.00	60997.97	39.43	60958.54	35165.08	876.72	55	–	–
56.82	61209.48	67.02	61142.46	67.02	61075.44	56 82	61018.62	37.96	60980.66	34288.36	887.45	56	–	–
54.59	61222.82	64.39	61158.43	64.40	61094.03	54 59	61039.44	36.47	61002.97	33400.91	897.37	57	–	–
52.33	61236.19	61.73	61174.46	61.73	61112.73	52.33	61060.40	34.96	61025.44	32503.54	647.12	58	–	–
50.70	61245.73	59.82	61185.91	59.81	61126.10	50.70	61075.40	33 88	61041.52	31856.42	261.52	Equal to deg. of long. on the equator.	58	42	47.4
50.05	61249.55	59.03	61190.52	59 03	61131.49	50.04	61081.45	33.44	61048.01	31594.90	918.04	59	–	–
47.74	61262.88	56.31	61206.57	56.30	61150.27	47.74	61102.53	31.90	61070.63	30676.86		60	–	–
											4729.49			
36.11	61327.99	42.59	61285.40	42.59	61242.81	36.11	61206.70	24.13	61182.57	25947.37	1297.48	65	–	–
33.05	61344.61	38.98	61305.63	38.99	61266.64	33.05	61233.59	22.09	61211.50	24649.89	3637.83	Deg. meas. in Lapland. 66	20	–
24.93	61387.72	29.41	61358.31	29.41	61328.90	24.93	61303.97	16.62	61287.35	21012.06		70	–	–
15.04	61438.34	17.74	61420.60	17.73	61402.87	15.04	61387.83	9.55	61377.28	15908.82	5103.24	75	–	–
7 36	61477.26	8.68	61468.58	8.68	61459.90	7.36	61452.54	4 92	61447.62	10677.85	5330.97	80	–	–
2.02	61499 75	2 38	61497.37	2.39	61494.98	2.02	61492.96	1.36	61491.60	5360.40	5317.45	85	–	–
0.00	61500.00	0.00	61500.00	0 00	61500.00	0.00	61500.00	0.00	61500.00	00.00	5360 40	90	–	–

XX. *An Account of Three Volcanos in the Moon.* *By* William Herſchel, *LL. D. F. R. S.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 26, 1787.

IT will be neceſſary to ſay a few words by way of introduction to the account I have to give of ſome appearances upon the moon, which I perceived the 19th and 20th of this month. The phænomena of nature, eſpecially thoſe that fall under the inſpection of the aſtronomer, are to be viewed, not only with the uſual attention to facts as they occur, but with the eye of reaſon and experience. In this we are however not allowed to depart from plain appearances; though their origin and ſignification ſhould be indicated by the moſt characteriſing features. Thus, when we ſee, on the ſurface of the moon, a great number of elevations, from half a mile to a mile and an half in height, we are ſtrictly intitled to call them mountains; but, when we attend to their particular ſhape, in which many of them reſemble the craters of our volcanos, and thence argue, that they owe their origin to the ſame cauſe which has modelled many of theſe, we may be ſaid to ſee by analogy, or with the eye of reaſon. Now, in this latter caſe, though it may be convenient, in ſpeaking of phænomena, to uſe expreſſions that can only be juſtified by reaſoning upon the facts themſelves, it will certainly be the ſafeſt way not to neglect a full deſcription of them, that it may appear to others

how far we have been authorized to uſe the mental eye. This being premiſed, I may ſafely proceed to give my obſervations.

April 19, 1787, 10 h. 36′ ſidereal time.

I perceive three volcanos in different places of the dark part of the new moon. Two of them are either already nearly extinct, or otherwiſe in a ſtate of going to break out; which perhaps may be decided next lunation. The third ſhews an actual eruption of fire, or luminous matter. I meaſured the diſtance of the crater from the northern limb of the moon, and found it 3′ 57″,3. Its light is much brighter than the nucleus of the comet which M. MECHAIN diſcovered at Paris the 10th of this month.

April 20, 1787, 10 h. 2′ ſidereal time.

The volcano burns with greater violence than laſt night. I believe its diameter cannot be leſs than 3″, by comparing it with that of the Georgian planet; as Jupiter was near at hand, I turned the teleſcope to his third ſatellite, and eſtimated the diameter of the burning part of the volcano to be equal to at leaſt twice that of the ſatellite. Hence we may compute that the ſhining or burning matter muſt be above three miles in diameter. It is of an irregular round figure, and very ſharply defined on the edges. The other two volcanos are much farther towards the center of the moon, and reſemble large, pretty faint nebulæ, that are gradually much brighter in the middle; but no well defined luminous ſpot can be diſcerned in them. Theſe three ſpots are plainly to be diſtinguiſhed from the reſt of the marks upon the moon; for the reflection of the

ſun's rays from the earth is, in its preſent ſituation, ſufficiently bright, with a ten-feet reflector, to ſhew the moon's ſpots, even the darkeſt of them: nor did I perceive any ſimilar phænomena laſt lunation, though I then viewed the ſame places with the ſame inſtrument.

The appearance of what I have called the actual fire or eruption of a volcano, exactly reſembled a ſmall piece of burning charcoal, when it is covered by a very thin coat of white aſhes, which frequently adhere to it when it has been ſome time ignited; and it had a degree of brightneſs, about as ſtrong as that with which ſuch a coal would be ſeen to glow in faint daylight.

All the adjacent parts of the volcanic mountain ſeemed to be faintly illuminated by the eruption, and were gradually more obſcure as they lay at a greater diſtance from the crater.

This eruption reſembled much that which I ſaw on the 4th of May, in the year 1783; an account of which, with many remarkable particulars relating to volcanic mountains in the moon, I ſhall take an early opportunity of communicating to this Society. It differed, however, conſiderably in magnitude and brightneſs; for the volcano of the year 1783, though much brighter than that which is now burning, was not nearly ſo large in the dimenſions of its eruption: The former ſeen in the teleſcope reſembled a ſtar of the fourth magnitude as it appears to the natural eye; this, on the contrary, ſhews a viſible diſk of luminous matter, very different from the ſparkling brightneſs of ſtar-light.

WILLIAM HERSCHEL.

Slough near Windſor,
April 21, 1787.

P. S.

P.S. M. MÉCHAIN having favoured me with an account of the diſcovery of his comet, I looked for it among the Pleiades, ſuppoſing its track ſince the 10th of this month to lie that way; and ſaw it April 19th, at 10 h. 10′ ſidereal time, when it preceded FL. *d* Pleiadum about 54″ in time, with nearly the ſame declination as that ſtar; but no great accuracy was attempted in the determination of its place. As I have mentioned the comet in a foregoing paragraph of this Paper, I thought it proper here to add my obſervation of it. "The "comet is nearly round, with a ſmall tail towards the north "following part; the chevelure extends to about four or five "minutes; and it has a central, very ſmall, ill-defined nu-"cleus, of no great brightneſs."

END OF PART I. OF VOL. LXXVII.